UNITED STATES DISTRICT COURT

EASTERN DISTRICT OF NEW YORK

BLUE CROSS AND BLUE SHIELD OF NEW)
JERSEY, INC., et al.,)
Plaintiffs,)
vs.) C.A. No. 98 Civ.
PHILIP MORRIS, INCORPORATED,) 3287 (JBW)
et al.,)
Defendants.)

COPY

The videotaped deposition of JAMES HECKMAN, Ph.D., called as a witness for examination, taken pursuant to the Federal Rules of Civil Procedure of the United States District Courts pertaining to the taking of depositions, taken before ANDREA I. CARTER, a Notary Public within and for the County of Cook, State of Illinois, and a Certified Shorthand Reporter of said state, CSR No. 84-3722, at Suite 2900, 30 North LaSalle Street, Chicago, Illinois, on the 17th day of February, A.D. 2001, at 9:06 a.m.

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PRESENT:

DEWEY BALLANTINE LLP,
(1301 Avenue of the Americas,
New York, New York 10019-6092), by:
MR. JOSEPH ANGLAND,
appeared on behalf of Plaintiff
Blue Cross/Blue Shield;

COLLIER SHANNON SCOTT, PLLC,
(3050 K Street, NW, Suite 400,
Washington, DC 20007-5108), by:
MR. JOHN B. WILLIAMS,
appeared on behalf of Defendant
R.J. Reynolds;

ARNOLD & PORTER,
(555 Twelfth Street, N.W.,
Washington, D.C., 20004-1202), by:
MR. MURRAY R. GARNICK,
appeared on behalf of Defendant
Philip Morris, Inc.

ALSO PRESENT:

MR. CARY DAVIDOW, Videographer.

REPORTED BY: ANDREA L. CARTER,
Illinois CSR No. 84-3722.

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THE VIDEOGRAPHER: We are going on the video record at 9:06 a.m. My name is Cary Davidow. I am a legal videographer in association with Esquire Video Services. My address is 155 North Wacker Drive, Chicago, Illinois. The court reporter today is Andrea Carter of Esquire Deposition Services. Here begins the videotape deposition of James Heckman taking place at 30 North LaSalle Street, Suite 2900, Chicago, Illinois. Today's date is February 17, 2001.

This deposition is being taken in the matter of Blue Cross/Blue Shield of New Jersey, Inc., et al. versus Philip Morris, Inc., et al. in the United States District Court, Eastern District of New York; Case No. 98 Civ. 3287 (JBW). This deposition is being taken on behalf of the plaintiffs. The party at whose insistence this deposition is being recorded on an audio-visual recording device are the plaintiffs.

Counsel please state their names for the record.

MR. ANGLAND: Joseph Angland of the firm of Dewey Ballantine representing plaintiffs.

MR. WILLIAMS: John Williams, Collier Shannon

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Scott representing R.J.R.

MR. GARNICK: Murray Garnick, Arnold & Porter representing Philip Morris.

THE VIDEOGRAPHER: And will the reporter swear in the witness, please.

(WHEREUPON, the witness was duly sworn.)

JAMES HECKMAN, Ph.D.,

called as a witness herein, having been first duly sworn, was examined and testified as follows:

EXAMINATION

BY MR. ANGLAND:

Q. Good morning, Professor Heckman.

A. Good morning.

Q. You have had your deposition taken before; is that correct?

A. That's correct.

Q. Right. So I am sure you understand the basic guidelines, but just to be clear, you understand that I will be asking you questions and you will be answering them under oath, the same sort of oath that would govern you if you were testifying at trial?

A. I understand.

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Q. If at any point you need a break, just let me know. I will be happy to accommodate you. And if there is any question I ask that you don't understand, could you please let me know; and I will do my best to try to rephrase it or to try to use your help to rephrase it so -- to render it somewhat understandable and intelligible. Okay?

A. Okay.

Q. Thank you.

Roughly, when were you retained to work on this litigation?

A. Well, that's a good question. In the sense of the exact date, I couldn't tell you, but it was in I think late -- mid to late 1999.

Q. Okay.

A. I wrote a report that was filed January 17th so obviously it was before that time, but I don't remember the exact date.

Q. What other tobacco litigations have you worked on?

A. I have worked on the case recently -- well, not concluded case or partially concluded case of Falise where I did some research on a case involving asbestos workers and interventions that

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were alleged to have effects on asbestos workers.

Q. Any other cases?

A. There's another case that's related that I am working on. I haven't really given a deposition or testified which is the National Asbestos Workers case.

Q. And have you worked on any other tobacco-related litigations other than this and the two you just mentioned?

A. Only indirectly in the sense that at one time I was brought in as a consultant to Lexecon, a general economist consultant, to discuss aspects of the settlement a few years ago. So I went to New York with several economists from the firm of Lexecon to have general discussions. These were very big think discussions; nothing specific; no specific testimony; no depositions; nothing given.

We were just talking out loud about aspects of the tobacco settlement that were on the table at that time, and I did research at that time looking at the determinants of smoking and looking at various data sets that had been put out on the table relating to the smoking behavior of Americans. So but it was very general. It wasn't

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specifically motivated towards litigation. I did meet with lawyers but no judges, no -- nothing really official.

Q. Okay. What was your assignment in this case?

A. My assignment in this case was to study the document that -- report, two reports really filed by Dr. Harris in regard to Blue Cross/Blue Shield. So there was a report that was written I believe around November 11th or filed November 11th, 1999 and another one filed November 29, 1999 where I was to examine the arguments or aspects of the arguments relating to behavior information and the like.

Q. What was the topic of the November 11th report of Dr. Harris?

A. That was a -- the report November 11th was a general discussion that he presented about his -- evidence about -- the report -- attempts to deceive by the tobacco companies, deceive consumers, deceive companies in general, yes.

Q. Let me back up for just a second.

At the time you wrote your report in this case, you said you had not been deposed or

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testified in any litigation in the prior five years?

A. That's correct.

Q. Since then, you have given both deposition and trial testimony --

A. That's correct.

Q. -- in the Falise case; is that correct?

A. In the Falise case, that's correct.

Q. Have you given testimony trial or depositions in any other cases?

A. Not tobacco cases.

Q. Well, first of all, tobacco cases, the answer are no on --

A. No.

Q. What about in nontobacco cases?

A. Yes, I have given testimony in a private antitrust case years ago. It was Sprint versus AT&T in Judge Richey's chambers in Washington, D.C. I believe around 1982. I was retained by the Justice Department -- a group of us at the University of Chicago actually were retained to do as study of antitrust in particular looking at the question of whether or not AT&T was a natural monopoly. And we were given a very fee license to

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do research on the question of anti -- of monopoly, and the case was settled without our input really, the case of U.S. versus AT&T.

But Sprint learned about our research and asked us if we could testify and I did, several of us did actually about just what our findings were. Our findings were actually published in scholarly journals so it was relatively easy testimony.

And then I have also given testimony in cases -- a few cases, very small potato cases, involving estimates of earnings losses from damages and so forth. Among other things, I do labor economics and look at lifetime earnings of individuals, but I haven't done that in 20 years. The last time I did it was up here in Waukegan. So that's -- my experience is relatively limited actually, quite limited I guess.

Q. In the Sprint versus AT&T case, did you address the damage issues at all?

A. I addressed a range of questions. The main focus though in the Sprint question was to see whether or not -- it was a very technically econometric question. It was simply to ask was the

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evidence on AT&T being a natural monopoly good evidence, and then there were implications that flowed out of that for various aspects of, you know, payments, damages; but my main focus was a technical focus which is simply looking at the question of whether or not there were any -- what the state of the econometric evidence was. So really not damages per se.

Q. Okay. That was more out of curiosity than anything else. Our firm handled that case for AT&T, and --

A. Oh, Sprint versus --

Q. Yeah. We tried that case for AT&T, and while I was only involved peripherally, I did get involved with the damage experts, and I had not recalled meeting you at that time, and it was --

A. No, this was more of a technical testimony. It was an interesting experience simply because the judge seemed quite well informed actually in that case I thought so.

Q. In the end we thought so.

A. Well, yes, I guess.

Q. How did you go about doing your chore -- discharging your chore in this case?

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A. Well, as I indicate in the report, my task was to study various statements made by Professor Harris, Dr. Harris, and to research the literature that supported that and other literature involving related issues, issues that were raised in the Harris report and issues that surround the claims and topics discussed in the Harris report.

So my approach was basically more of a secondary research plus primary analysis as an analytical economist, but my task was to read the literature, become conversant with the details in the literature, of course, to read the Harris report and then to form my own judgment of what I thought the evidence showed and maybe in some sense where I might disagree with Dr. Harris about certain readings of the evidence.

But partly my task was to become familiar with a very specific literature. Now, I had become familiar with general aspects of the smoking and on-set of smoking literature, but there were a number of references and statements made that required specific reading of the literature. So it was primarily a secondary researcher.

Q. With respect to the literature that you

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read in discharging your -- in doing your work in this case, most of that literature -- for most of that literature was it the first time that you had read it?

A. No, only because I had been looking at this in regard to the settlement cases.

Q. Okay.

A. And because I had several dissertation students at Chicago. I don't know if you want me to digress, and you can shut me up whenever you want. But in terms of my reading of the literature on addiction and smoking generally, a colleague of mine, Gary Becker, who is cited in this literature and a former student of mine -- a joint student of ours actually Kevin Murphy had written a paper on addiction going back to the '80s.

So for quite a while around the Chicago environment, there was a very big discussion of addiction, various kinds of addiction not just smoking. In specific cases though, I -- so a lot of seminars. Jan Elster, who is now at Columbia, but at that time was at the University of Chicago when Becker was starting his work, conducted a whole series of seminars, sort of anti-Becker

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seminars on smoking and addiction rationality; and I attended those and participated in several discussion, just colloquia.

In the course of doing that, since I am an empirical economist, my interests were drawn into reading the econometric studies. So it wasn't my first acquaintance. So when I looked at this literature -- and then, of course, specifically I had several dissertation students who worked on addiction, and a particular person named John Cawley who is now at the University of Michigan who looked at not smoking, but the question of addiction and weight and he -- and I looked at this. And, in fact, he came with me to several of the meetings at Lexecon where we were discussing the big issues on settlement.

So in that sense it wasn't new, but there were certainly some aspects of the interventions which I had not seen prior to the time I studied this.

Q. Sort of an irrelevant aside, but was Professor Becker at Wisconsin at one point?

A. No. There is another person named Howard Becker I think who was at -- but, no.

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Milton Friedman was at Wisconsin but never -- never --

Q. All these Nobel laureates run together.

A. No, actually, there's a very ugly story about Milton Friedman at Wisconsin. In fact, the Wisconsin history has this ugly little aside. They have a whole section of why Milton Friedman wasn't given tenure at Wisconsin.

But, no, not Becker to my knowledge.

Q. Now, you described doing a review of the literature in connection with your work in this case?

A. Yes.

Q. And that included empirical literature; is that right?

A. Very much so.

Q. Okay. Did you do any empirical work of your own in this case?

A. Well, in the sense that I had looked -- well, let me step back.

In the sense of filing this report, the answer is no.

Q. Okay.

A. What I did do, however, later and in

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relationship to National Asbestos Workers case was to re-examine some of the data, primary data. So in some sense the broader question is, yes, but for this report, no. So what's reported here it would predate my -- any empirical work that I had done.

Q. Do you intend to rely for your conclusions in this case upon any of the empirical work that you did for National Asbestos?

A. Well, not specifically. Only in a sense of gaining a general impression. It certainly helped me understand the structure of the data that were being used here in the prevalence model of Dr. Harris. To go back and reanalyze it, I understood more -- one always gains experience in doing empirical work by looking at the data one's self, computing regressions, doing sensitivity studies, which is what I did in the asbestos workers case. This are not reported here in any -- and does not show up in any of the reports here -- this report here, I should say.

Q. Is the work that you are talking about work that does show up in the form of a report in National Asbestos?

A. Yes. There is a report and a

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supplementary report where we analyzed the data from the National -- from the same data that was used here, the Harris reports.

Q. And which data is that or are that?

A. Well, there are several data sets that are used in the Harris reports including CPS data, data from NHANES and so forth. So we have a variety of pool data sets that constitute a common -- that constitute the data base that Harris uses to estimate his models. I have the National Asbestos Worker report if you wanted the exact names of the data sets.

Q. I might take you up on that in a bit, but I am just trying to get a better general understanding now.

A. Yes.

Q. You refer to the CPS data set?

A. The Current Population Survey on Smoking, yes.

Q. Okay. I was going to -- you have answered my question. There's also a Cancer Prevention Study, CPS.

A. Okay.

Q. And I was a little confused as to how

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that related to this, but I understand your point.

Can you describe in general terms what your conclusions were based upon your analysis of these data sets with the National Asbestos case?

A. Well, first of all, I tested a number of assumptions that were made in the Harris model. I looked specifically at the structure of the data. It's a time series that he constructs starting back from the early '50s and running up into the '80s, and that data set is -- consists of several features: One, you are pooling together different data sets, which is always potentially problematic.

More problematic in my thinking was the absence of very many observations at the beginning of the sample period, and so there is a real question that bothered me -- that bothers me, I should say, because it still remains an issue as to what information was really out there in the period in the '50s which are a crucial period for looking at the first smoking scare.

So I examined the assumptions that were used to essentially use that information and to use the information from the post-60s, '64 period really -- '64-plus period to build a model to

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extrapolate back and to somehow build a structure on the underlying period where there was skimpy data. So I was worried about whether or not there was much information in the early period.

So one question I have is -- and one important piece of analysis that was revealed was that the data in the early '50s really critically depended -- the inferences you draw are critically dependent on the assumptions made about the post-60s period, and some of those assumptions are tested and relaxed. They produce a different pattern of data. Some of which relating closer to the reports that actually appear in the Surgeon General's reports about onset of smoking, initiation and prevalence behavior.

So there was a real question. So the central question -- I could go through a lot of detail, if you wish. I would like to look at the report for a second just to refresh my memory, but I have looked at it recently. The issue is more that -- it's the missing data in the first ten years. There really aren't -- there are only two observations, and two observations is a dangerous business in econometric analysis and empirical

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work, and so, therefore, that attracted a lot of my attention.

Q. Did you propose an alternative model that gave your best estimate of what the shape of the curve was from 1954 up until about ten years later?

A. Well, not a single alternative, but what I did do was I tested a lot of the assumptions, you know, in terms of the regression models were making certain kinds of assumptions about quadratic equations, quadratic shapes of regression functions. And there was a real issue about not only the date of which the quadratic would start, you know, these are splines. They were broken up in different periods, but in addition it would be -- it would depend on what else was being assumed and how confident you were in pooling the different data sets together.

So I didn't report a single estimate, but in looking at the prevalence estimates, I find I do report a series of alternative estimates to examine how sensitive the assumptions are and how sensitive the estimates are to the assumptions made.

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Q. And just to clarify this in my own mind, were you simply doing a sensitivity analysis to show how sensitive the results are -- were to where you chose the not point or what form of curve was used or were you on the other hand -- and I realize you might have done both -- saying having looked at all this, I think the better model would be structured this way, and here are the results I get?

A. Well, in the sense that the -- there's a little of both in the sense that certainly after looking at the data that way, I felt there were things that could be done with the model that I report in that National Asbestos Worker supplement that would actually improve the fit of the model and that would relax some of the stronger assumptions.

But there was a more problematic aspect of that report on the whole analysis of the data, and that was in the absence of any information, what frightened me and concerned me greatly was the fact that I could get lots of stories that were consistent with the data.

And you could say, well, if I assume

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this, then I get that, if I assume this. The fact of the matter is in the absence of data, it's very difficult to -- it leaves plenty of room for speculation. So I didn't come up with a last model saying this is the best model because I face the same problem that Dr. Harris faces; namely, absence of data at the beginning of the sample. What I did do was show the sensitivity of the assumptions. And then I did certain improvements on the model. For example, you know, we have rates. Rates have to be numbers generally, you know, fractions. So what we are talking about is proportions, for example, on the prevalence data. They are just the raw data.

Prevalences can't go negative and they can't be greater than 100 percent. So, you know, you put restrictions on that by using transformations, logit-like transformations, transformations on the model and, you know, adjust for serial correlation, adjust for a whole series of econometric exercises; and I think it gets rid of some of the extreme -- some of the problems that come from extending the model over long periods of time.

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So looking prospectively, not retrospectively, I think you can probably come up with a better model in the sense that it's more plausible and gets out of some embarrassments that would be. So in that sense, I improved it, but I couldn't get around the fundamental problem of missing data in the first decade of the study.

Q. With respect to the first decade --

A. Yes.

Q. -- did you conclude that there was -- let me be clear.

I understand your point that given the shortness of data, there is a limit to how much could be done. I mean, is that fair?

A. Well, it's not just the shortness. It's the incompleteness. Yeah, it's that you literally have a gap -- I mean, there are some years where we have data, multiple data sources in the '70s and '80s where you can actually pile together three surveys and say, Ah, well, the levels are different but now I can track them.

You know, the time series move the same way. People feel comfortable. Here we have a gap of ten years. It's like Nixon's missing tapes.

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Q. Right.

A. 20 minutes are gone, and only in this case it's eight years, eight, nine years.

Q. Right.

A. And so it's a short series, but it's more than a short series. It's the missing part of it. So the series really starts in the '60s and that's terrible for the sense of saying, Well, what happened in the period of the '50s. That's the biggest problem, the Achilles' heel.

Q. Right. You have there I think a data point for 1954 --

A. Correct.

Q. -- and then no data points until some time in the early '60s --

A. '62 or so, yeah.

Q. Okay. Given the inherent problem that that causes that you have articulated well, was there a better approach do you think could have been used to estimating the shape of the curve during that '54 to '62 approach?

A. Well, in terms of a -- again, you see with missing data, you are asking a question what it is could have been, and then it's a whole

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question of the science of building a counterfactual world which is what is central to this case, the Blue Cross/Blue Shield and all these cases. And so the question is how closely can you be guided. Remember the '50s were a period of great turmoil in the cigarette industry generally, the tobacco industry. And in particularly this is the time when the first indications are made of smoking and health risks. You have health scares in '54, and you have sort of an early report, early statement by the Surgeon General in '57 and activity that appears in the journals and scientific activity and some publicity.

So to get to the point, can you do better? Well, only to a limited extent. What you can do is show sensitivity, but in the sense of saying that there are missing data, it's difficult especially when there are missing time series data. It's not like missing a few observations here for people of this education class. It's like you are missing a central part of your story.

So improving it? No, I have my doubts. Just simply this is a case where we don't have data. We don't have data for example on -- to

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take a totally different area. We don't have data on earnings related to education in 1930 or 1920. The census didn't collect that information until 1940. So what happens is you have to speculate, and that's been the subject of some speculation about what the actual rate of return to education was.

Q. That was one of Dr. Becker's big areas of work, wasn't it?

A. That's correct, and recently has received a lot of attention because of the changing inequality in the labor market. So you have this huge change in inequality, and people wonder what happened in the first half of the century. Since you don't have any data until 1940, you have to just piece it together.

I don't know if every single data set has been exhausted. I do know and I have looked at reports that suggest, for example, that the Milwaukee Journal had time series on smoking, and so I am not sure. So it's one question that intrigues me that there might be more data out there to look it. I have not gotten the Milwaukee Journal data sets, but there are other sets --

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data sets. But again, you know, the Milwaukee Journal is not the U.S. Census or any of these government agencies that survey. So anyway, I think there are probably more data to be gathered and that would help a lot in this question for sure.

Q. Do you know Dr. Harris personally?

A. I have met Dr. Harris on several occasions. I gave a week's worth of lectures at MIT in 1981 I think it was. I was invited in. They have a program at MIT to invite an outsider in during their break -- break -- winter break, January, and I was invited in actually by McFadden, my co-recipient on this prize, to give a week's worth of lectures at MIT. And Jeffery Harris was there and we spoke a lot and we had mutual interests -- have mutual interests, but we had mutual interests. We were both concerned with a so-called problem of population heterogeneity.

So he was looking at what the doctors call frailty models and mixture models and whatever, and we shared -- we had several discussions at that time. And subsequent to that I have had -- exchanged letters and papers, and I

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was editor of the Journal of Political Economy and used him as a referee for papers so.

Q. Now, I understand you disagree with a number of the conclusions Dr. Harris drew as reflected in your report?

A. Yes.

Q. Acknowledging that, do you have any view regarding his competence as an economist?

MR. WILLIAMS: Object to the form.

THE WITNESS: Should I answer?

BY MR. ANGLAND:

Q. Yes.

A. Well, first of all, I think I have tracked Dr. Harris' record -- well, his career for 25 years because he was doing more than medical economics. He was also doing some interesting work with Michael Wachter on the employment relationship. Wachter is at Pen and in the law school at Pen and in the department, and my opinion of him based on all my academic experience with him through all the academic literature was that his scholarly papers, the refereed scholarly papers were very good.

I think he is a major figure in health

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economics. I found his work on heterogeneity to be quite good. I found the work on the employment relationship to be quite doubt. I don't think there's any doubt he is an established and important figure in medical economics.

Q. Have you looked at the expert reports submitted by any of the other experts for the defendants, the tobacco companies, in this litigation?

A. I have looked at some of the reports.

Q. Which ones?

A. I looked in particular at a report -- I think it's been submitted. You have to correct me now. I have looked at reports by English which was on the medical history, and I did look at Wecker's report, and I looked at a report by Donald Rubin that was filed in relationship to his methods of missing data. Those are the main reports, and I think there's another report by DiBaccio, but I am not sure that was filed.

Q. So we will pass on DiBaccio anyhow.

A. Yeah.

Q. Since I haven't read it, I can't ask you anything --

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A. I don't know anything about the status of that report, but those are the three. Although the English report I only read quite recently. It's an interesting piece of history.

Q. With respect to the Wecker report, --

A. Yes.

Q. -- was there anything in that report that you found you had reservations about or disagreed with?

A. Well, I have to look at it really closely again because that was something I did look at when it came out in March. I think it was March of 2000, not sure.

MR. WILLIAMS: Why don't you show it to him and ask him specific questions.

THE WITNESS: Yeah, that would be helpful to me.

MR. ANGLAND: I don't have it with me. I am asking what he recalls.

MR. WILLIAMS: Well, he said he hadn't seen it in a year.

THE WITNESS: Yeah, close to a year. So I hesitate to --

BY MR. ANGLAND:

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Q. Just to be clear, I mean, this isn't a memory test, and I realize that --

A. Okay, yeah.

Q. -- you know, that you may if it were in front of you have other things to say. I am just asking for you what you recall, and I realize that may not be exhaustive of what you believed at the time.

A. Yeah, I just -- I am not trying to be coy with you.

Q. I appreciate that.

A. It's literally a situation where I would have to look at it to be clear, and I will just say this. Wecker and I were colleagues here at Chicago and I knew him pretty well and I kind of liked him in the sense that I thought he was a very hardnosed applied statistician. So when he was here at Chicago Business School -- here, being at the University of Chicago -- I always liked him because he was hardnosed, critical, kind of a scrappy character, but -- and so I remember at the time feeling general agreement but that's a vague statement.

Q. What about with respect to Professor

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Rubin's report; do you recall having reservations or disagreements with anything he said?

A. Well, I thought that the Rubin report -- again, it's been a while since I looked at it closely. Pieces of it I remember. We were talking about -- he was talking about imputing missing data and questioning I believe Dr. Harris although this is way off the topic. I do remember he was talking about propensity score estimation, and I didn't fundamentally disagree with him. I think we have different weights that we might place on the importance of that topic, but it's still -- I didn't find anything wrong with his study that I saw.

Q. In what way does your view, the weight to be attributed to propensity scoring differ from Dr. Rubin's?

A. Well, we wrote papers together in a volume that was published in 1986 by Howard Weaner. It's actually just been reissued this volume, and it's called, Inference From Self-Selected Samples, and I had only -- Don Rubin and I were colleagues together at the University of Chicago for about four or five years. And at the time that I wrote

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the study, I believe he had just gone to Harvard, but I am not sure. He may still have been on the faculty. We did talk a lot and exchanged ideas.

I had developed certain methods for missing data and he had developed certain methods, and what was interesting to me was that both methods relied on this probability of the data being missing but they used the information in different ways. And so in that sense we -- it was an interesting comparison to see how the different methods -- what one method assumed and what one method didn't assume and the limitations of the each of the methods and the alternative identifying assumptions.

So that was the nature of our exchange. So when I looked at -- so my view on the propensity score method was interesting, that's a clever idea, and it does solve the missing data problem under some important and widely used assumptions. In fact, the kind of assumptions that Dr. Harrison was making. So I thought that -- at the time as I recall, Dr. Rubin's analysis of propensity score methods struck me as being a relatively useful way to essentially reduce the dimension of the kind of

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problem that Harrison was facing with, you know, all these missing observations. I think there were 41 variables. I can't remember. Harris had a lot of data that were -- and the beauty of the propensity score method is that it allows you to solve the missing data problem under the kind of Harrison assumptions using a very simple statistic rather than a high dimensional regression. So I thought -- I thought in that sense it was a good application of the propensity score method what Rubin was doing.

Q. Now, in what sense is the problem that propensity scores is designed to deal with a missing data problem?

A. Well, in the following sense: That first of all, the way propensity scores are usually considered, they are considered as tools for looking at something like a treatment, a control problem like in a medical evaluation; and the critical assumption that's made in that literature, an assumption that many epidemiologists make and I do read the epidemiology literature and contribute to it although my relationship is probably closest with a person named Jamie Robins who is at the

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School of Public Health and Brigham and Williams -- Women's Hospital in Boston.

But in that literature, there's a strong assumption made on what's called -- so literally the point is you have a counterfactual state you are trying to create, and the crucial assumption is if I can somehow control for the differences between two groups, those who got the treatment and those who didn't, I want to make sure that I know that the difference that I attribute to the treatment is really due to the treatment rather than some pre-existing difference that would exist anyway.

So the Harrison idea and the idea in a lot of studies -- I shouldn't say Harrison -- Rubin and a number of studies is basically to say, look, if I can put enough X's in, enough control variables, so sort of make people as alike as possible on what I can observe, then I can get an unbiased or at least large sample unbiased estimate of the effect of the treatment that's not confounded by a set of remitted variables, and so I include those variables.

And the beauty of the propensity score

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method is that it allows me to introduce all those mini-variables through one score, the P, the propensity scores. And so it let's me reduce the dimension of the matching problem of sort of trying -- so but the crucial idea in this whole counterfactual business is to control for selective differences that would have existed even in the absence of any treatment effects.

So, you know, if I assign treatment to extremely sick people and my control group are very healthy people, then by definition I can get very bad effects for the treatment because even though it actually improved their status, the people they are being compared with were quite healthy. And so the idea is to, you know, construct ways to control for that differential bias between the two groups.

Q. You talked in your answer, and I forgot your exact words, but about picking the samples or matching the samples?

A. Yes.

Q. One use of propensity scores as I understand it is when you are doing a matched sample based experiment --

A. Yes.

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Q. -- to chose people for the control group versus the treatment group, correct?

A. Correct.

Q. In the context of this case and perhaps other context, Professor Rubin uses propensity scores in a slightly different way which is using them as a diagnostic tool in a case where you are using not a matched sample but you are doing a cross-sectional analysis?

A. Right. But the two are closely related and this is why because what happens is that at the end of the day -- take Harrison's study in particular, which I did skim. I don't want to set myself up as an expert, but it's a very common problem when you have missing data, and so what you do is you build a model where you are predicting the outcome using so-called X variables, the covariates, and you can -- the assumption is that you can essentially use the data on the people you do observe to build a regression model to impute values for those you don't observe. That's the key idea.

But you see what you are really have if you want to think about it is a kind of treatment

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control problem. You can think of the treatment as being the people who have missing data and the control is those who are -- you have good data on, and you to -- you know, you want to say, well, can I borrow from the people I don't have data on -- I do have data on to infer information about the missing data for people I don't. Analytically it's the same as an evaluation problem of a drug because you have missing data because you don't really know for the treated people what their -- what their status would have been like if they hadn't been treated. So you have missing data for them.

So in both problems you are doing an imputation, and so in that sense there's a commonality of the missing data problem and so forth. So what Rubin was doing, as I recall, again I am stretching my memory a little bit. But Rubin as I recall was essentially saying, Look, Harrison is writing down a very complicated multivariate regression model using linearity, various kinds of assumptions. If it turns out that he is really -- you could get by in a non-parametric sense without making any of the strong functional form assumptions and getting on a lower dimensional data

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set by using P, using the propensity score, that should be just as good for sort of imputing the data.

So if I just, you know, looking at the missing -- so in other words, as a check for how good the Harrison procedure was, I could then use a propensity score-type matching method, imputation method to say those people have the same P. I am going to find what their Y's are like and then I am going to the use those Y's for people who are -- you know, have the same P but have a missing Y for them in the other sample.

So in that sense it's like going back and doing matching in reverse, and saying, yeah, I can use -- I can borrow strength, but I do it with one dimension, not multiple dimensions. So just ironically as you mentioned as I was talking to somebody in Sweden by E-mail just three days ago about this very question. His was a question of looking at the determinants of different political regimes in countries around the word and what their consequences were, but he was trying to model how countries would choose different regimes, and he had a high dimensional problem, a lot of

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regressors: Very few countries, small time series. So the idea was to use the P to do this kind of matching exercise to essentially reduce the dimension down.

And the point is is that under the assumptions that are made by people who say that I can control for the missing data problem by using the X's, the covariates, the predictors, that the dimension reduction of the P score will give me an alternative answer. So in that sense it's like a standard statistical method, not that the method is standard, but to say that what I am doing is saying, yes, there's a structure here which I can solve by a complicated method, there's a simpler method, do the answers agree. If they disagree, you would start to worry.

So in that sense I thought it was a useful -- a clever use of the propensity score method. Even though nominally it's not the same problem, for all practical purposes it is. See, the missing data problem and the evaluation problem for a drug is the same simply because you are missing the data on the people who weren't treated. You know, and so on the people who were untreated

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you don't know what they would have got if they were treated. If we can somehow compare these people and put them in both states, then the problem would be over, but that's a missing data problem. So you are imputing always missing data to people, and then it's very similar to the kind of question that Harrison was imputing and the scores are the same.

Q. In the context of observational -- first of all, Dr. Harrison's study you would characterize as an observational study; is that right?

A. Yes, yes. Oh, for sure, not an experiment.

Q. In the context of observational studies, how common is it in the literature to use propensity scoring as a diagnostic tool?

A. Well, first of all, propensity scoring was introduced in the '80s. If you look at the -- you have to say the literature. There are a bunch of different literatures. If you ask -- in the statistics, in biomedical literature it's getting to be a huge industry. In fact, it has become a very big industry in that -- so, for example, my

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associate, friend Robins who has his own views about these things certainly would use a propensity score as a means of solving this problem.

Among econometricians the method has not been as widely used. However, it is gaining use. I mean, I have contributed somewhat to this literature building it into standard econometric frameworks, and then comparing it to other models. So the answer is increasingly being used as a diagnostic in the sense of saying, well, you know, can I balance -- I mean, for example, I was at a conference in Mannheim, Germany in June of 1999, and an Italian scholar whose name I would have to dig up was using the propensity scoring exactly this way. And actually at the time I thought it was a little peculiar, and then I realized, I mean, right afterwards I saw exactly what he was doing.

His job was basically to reduce the dimensionality, making a comparison. A high dimensional problem and boil it down to a one-dimensional problem which is the P score, and then he used it as a diagnostic in the same way. So common, no, but new methods are -- you know, always take a while, and there are some technical

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issues that arise about applying the propensity score, and those have been worked out and people are becoming familiar with them now. So in that regard there's a -- you know, there's an emerging literature. So common, no, it's not every day but it's used a lot of in biostatistics.

Q. When did you first publish your truncation method?

A. Oh, I published that in 1976.

Q. Is it widely used?

A. Yes.

Q. Was it widely used ten years ago?

A. Ten years ago?

Q. Yes.

A. Yes.

Q. Was it widely used 20 years ago?

A. Starting to be.

Q. Okay. The Angland truncation method is going to be to try to get us all to airplanes by stopping this deposition at some pont. So that's -- that's my model for today.

MR. WILLIAMS: We hope it will be widely used.

BY MR. ANGLAND:

Q. If one were to look at the leading

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econometric journals and look at the observational studies that are reported in them over the last decade, would even 1 percent of them use propensity scoring as a diagnostic tool?

A. No. To be honest, no, not right now; but again, you know, you have got to be careful because there's always inertia. And if you look at the unpublished working papers that are being circulated and like the E-mail that I get from this guy at Sweden. I actually saw him in December, and we were -- he was raising this question to me. And if you look at the work that some of my students have had. I am running a project now and finished up a project I should say in Latin America where we used these methods for looking at job training programs in a number of countries in Latin American, that there is a growing interest.

So it's a question sort of asking is a backlog, no, it's not. It took a long while. My paper comparing selection and propensity score methods was put out in -- was published in '86, and I have been amazed at how slow people were to sort of catch on. And now more recently people are catching on and using it and, of course, there's

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always misuse of any statistical method. So I am not saying it's 100 percent. I wouldn't endorse every paper that uses it.

But I think you are right that the published literature over the last ten years would show not a very common use, but I would still say it's accepted. And if you looked at the statistics journals like the Journal of the American Statistical Association, Biometrika, biometrics, the journals in epidemiology, then the answer is quite a few, quite a few. It becomes almost a requirement in some circles, in medical circles. So in that sense, I think it's -- there's a gap between econometrics and the medical literature.

Q. Let me reask the question about the econometric literature still but confine myself to the last two years.

A. Yes.

Q. Do even 1 percent of the published observational studies during that period use the propensity score technique as a diagnostic tool?

A. 1 percent? Well, I mean, you have got to define the population for me, right. I mean, 1 percent of all papers? I mean, if I were to ask

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what does the standard tool that econometricians and applied economists are using, it would be probably a method called instrumental variables more than the propensity score method or any other method in terms of correcting right now. In natural experiments, literature is literature that's basically an instrumental variables literature. So, yes, there's a very -- so 1 percent I might squeeze it in.

Q. But not 5 percent?

A. No. No, not in the econometrics literature, but again I stress that in the statistics literature, the roles would be reversed. See IV is being used as a new innovation in the -- in the statistics literature which is kind of -- so now you are getting a few papers -- it's almost the mirror image of each other. You get like, you know, 5 percent -- 98 percent of all the studies in the -- in the -- in the -- well, I would say mirror image, but I would say there's a very different emphasis in the two literatures.

Q. Now, to switch to the statistical literature, let me put for aside the use of propensity scoring to creat matched samples.

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A. Yes.

Q. And instead look at observational studies and the use of propensity scoring as a diagnostic tool to test the validity or reasonableness of conclusions of an observational study. Okay?

A. Yes.

Q. For that use, the use as a diagnostic tool, what percentage -- if we were to look at the last ten years in the statistical literature, what percentage of the observational studies reported there would you estimate to use propensity scoring as a diagnostic tool?

A. More. It's not zero. It's not zero.

Q. It's not 10 percent though, is it?

A. Well, I can't claim that I know every study in statistics. I mean, I read a bunch of those journals, but it's catch as catch can. I do read in this area.

No, it's not 10 percent.

Q. Okay.

A. But that doesn't mean that it won't be used. I mean, to me it's a very important tool, and I think people have been slow to understand --

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so I would endorse it. And I think it actually is a clever way to do a test of -- you know, you have a high dimensional model.

So it's a little bit like saying, you know, in 1923 how many people used relativity. You know, not many but it's coming; and in that sense, I think it's reputable. I don't think it's something --

Q. Do you use it?

A. Well, I would use it. I would use it, yeah.

Q. Have you used it in your published work?

A. Yes. Oh, yes.

Q. Which work?

A. Oh, I have a paper published -- several papers published. The Econometrika in 1998 paper with Todd and Smith. There we actually use it as a comparison with -- we used propensity methods and our extensions of propensity methods, selection methods and experimental methods to evaluate a particular -- review a particular job training program that was originally designed experimentally.

So my objective there was to say how

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well done -- well, first of all, assuming the experiment was well done, how well do other methods work. And so, yes, I used propensity score method as a means of comparison. The Review of Economic Studies article did the same. I had a paper in the National Academy of Sciences proceedings in 1996. So I have used it. Yeah, I have used it for sure.

Q. To what extent is propensity scoring addressed in the leading econometric textbooks? And just to make it a more reasonable question in some sense, that have been published in the last five years.

A. Oh, I think it's probably not there, but don't forget the average econometric's textbook is trailing the frontier of knowledge probably 10 to 15 years. I mean, the average one, the ones that you see for undergraduates and graduate -- first-year graduate students. I don't even use the first-year graduate student books anymore in my class because they are so out of date.

So but, no, they wouldn't. I do not know of any undergraduate or graduate first-year book that uses the propensity score method. Now, coming along the way is a paper -- is a book by

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Jeff Wooldrige at Michigan State who is going to have a small section where he does mention propensity score methods. That book is either about to be published or maybe even is published. I have seen the preprint. I haven't seen the final version.

Q. Do you have some involvement in a Handbook of Econometrics?

A. I am the editor.

Q. That's what -- I thought I was straining my memory there.

A. Yes. A co-editor with Edward Leamer I should say.

Q. What's the target audience of that publication?

A. Oh, that's professional econometricians and generally applied -- sophisticated applied people. In other words, the handbook series has as its target a group of certainly econometric theorists, but people maybe not at the best schools, just around the world. My image is always somebody sitting in a jungle in the Congo or something. You know, if they want to find out about something, they will read about it. But

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also, you know, the informed person in another field who just wants to get sophisticated and know what the references are. So that's the target, yes.

Q. Is that a more advanced or less advanced target than the first-year graduate econometrics one?

A. Probably somewhat more advanced, somewhat more advanced, a little bit more, yeah.

Q. Okay. Who did the treatment of propensity scoring in the Handbook of Econometrics?

A. I did.

Q. Okay.

A. I had a chapter on that.

Q. Right, and --

A. Well, now wait, be careful. That's under preparation. Yes, right.

Q. I was just about to clarify.

A. Okay. Right. No, there were four volumes out now in the Handbook of Econometric series. Three are edited by Griliches and Intriligator and the fourth is -- was edited by McFadden and Rob Engle, and the fifth -- and there are parts of these. So the fifth is, you know, I

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don't know how many volumes 5A and 5B so. But anyway, the fifth is half-published.

So my handbook is halfway done in the sense that I have had incredible trouble getting my co-authors to finish their papers. There's nothing you can do to get people to finish things. So that -- and in the earlier issues, nothing would have been mentioned on the propensity score, nothing. The only mention is in a handbook of -- is in Labor Econometrics in an essay that I wrote with MaCurdy where we just have a footnote on propensity score. So but it's not mentioned. I agree.

Q. Okay. When was that prior edition done?

A. Well, there were several. One was like published in '85, '86, '87 I think roughly. I don't want to -- I think maybe the first was '84. My volume -- so I guess it's '84, '85, '86. And then there was another volume that was published in '93 or '94. That's the McFadden Engle, and then my first volume is coming out in 2001 and hopefully the second will be out 2002. I will be lynched if I don't get it out actually.

Q. Do you know Dr. Franklin Fisher at MIT?

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A. Sure, I have met him. I have known him for years.

Q. Do you have any view of his competence as an econometrician?

MR. WILLIAMS: Form objection.

BY THE WITNESS:

A. Yeah, Franklin Fisher is a first rate mind. I think he's written -- I mean, when I was a graduate student, he was well established. His book on the identification problem is classic. I think he has done a lot of important papers over the years. I think he is somebody I admire actually.

BY MR. ANGLAND:

Q. Putting together the work you have done on this case, the Falice case and the National Asbestos case, can you estimate the number of hours you have spent working?

A. I could give you an exact total if I had come prepared for it. I can give you a rough estimate.

Q. A rough estimate is fine.

A. So we are going back maybe -- again, that's the question of the starting date.

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Certainly by the mid summer of '99 I remember conversations, but initially the input was relatively low. So I could give you a number. I am not sure it would mean much. I can give you a rough idea.

Q. I will let you give me a confidence interval after.

A. Well, I am not sure it's a confidence interval. I can give you a number. In the period in which I was reading the literature, there was a very intensive time input. It could be -- but again, you know, these papers are relatively -- many of the papers are relatively easy to read but still so. I recollect over a period of time -- again, there were periods of intense bouts where one would work like eight, nine hours a day for four or five days, and I did have such periods. So that would give me at least now 30, 40 hours, you know, on a couple of these cases.

But generally speaking if I were to sort of put down a monthly -- monthly input, it would probably be somewhere maybe 20, 25 hours in a given month would be typical maybe, a little bit more in some months. Could be up to 30, 35 which is like a

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week's work maybe in the course of a month but not even that every month. But then in some months, for example, in preparing this report and reading the literature, I would just lock myself away, sit down with all the journal articles, go through and study and write reports; and that would be like 40-hour weeks.

Q. Okay.

A. So I am not trying to the coy. I just --

Q. No, that's certainly specific enough.

A. I could get the information but I don't know.

Q. Did you work with Lexecon in connection with these activities?

A. For sure I worked with Lexecon.

Q. And just for the record, Lexecon is an economic consulting group based in Chicago?

A. Yes, it is. It's located on Michigan Avenue.

Q. Who at Lexecon were you working with?

A. I was working primarily with Lisa Landes, a former student of mine actually at Columbia. She was a student. I have known her and

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her husband for close to 30 years as well as two -- another former student of mine Rick Flyer who is a recent -- fairly recent Chicago Ph.D. and Colleen Loughlin who is an economist trained at the University of Michigan who works at Lexecon.

Q. What services was Lexecon performing?

A. Well, --

Q. And if it will simplify --

A. Sure.

Q. -- if there were services unique to other cases, you can leave them out right now. So it would be work for this case or work that spanned the cases.

A. No, what we do in Lexecon is frequently would have discussions, bounce ideas. I would try something out. Some people would respond, object. They were more like bull sessions, more like seminars. They were not -- and so their task, if they had tasks, would also be to more or less check my accuracy. I would make claims. They would question them. We would both read the Harris reports. We would reach a conclusion, and we didn't always agree. What was written here I wrote, but nonetheless it's the structure of the

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document that -- that there was a lot of discussion because there are a lot of open issues surrounding these cases.

So we talked about this and addiction questions generally. So their task was here in this particular report was to provide me with an audience really, a background, a seminar for thinking which I find very helpful.

Q. Now, I know some of your work has touched on health economics. I state that as a premise, but apart from your work in this litigation -- well, strike that.

Do you consider yourself an expert on health economics as such?

A. Well, if you ask do I go to health economics meetings, the answer is no. Do I use the methods and develop methods that are used by health economists, yes. But, no, I am not a health economist in the sense that -- I am not a member of the National Academy of Medicine which virtually every major health economist is or quite a few of them seem to be, and I don't write papers that are directly related to health issues. However, fertility and mortality issues I have looked at.

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So that's health.

Q. Let's talk a little bit about rational addiction.

A. Yes.

Q. You referred earlier I think to the Becker-Murphy framework --

A. Correct.

Q. -- for analyzing rational addiction?

A. Correct.

Q. Now, in that framework the likelihood that an individual will engage in the addictive activity is a function of the individual's -- a function of among other thing, the individual's perception of the negative consequences over time of engaging in that activity; is that correct?

A. In their paper in particular they have a model of agents having perfect information about their preferences and making estimates of their price effects, for example. So they have an estimate -- in their particular framework, they can essentially -- they essentially assume that agents know their own preferences and they have a lot of the information available about the prices and so forth to solve the problem, that's correct.

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It's a perfect foresight.

Q. Okay. And --

A. Although it has been extended.

Q. I was about to say people using that framework have relaxed the assumption of perfect knowledge?

A. Correct.

Q. And reflected the possibility that somebody might over or underestimate the long-term adverse effects of the addicted behavior, correct?

A. Well, yes, what people have done is -- you are thinking of this paper by Orphanides and Zervos --

Q. Yes.

A. -- in particular where they have tried and successfully tried I think to sort of build in the notion that agents are experimenting, and like many people experimenting job search or migration search, they are taking information out from the system and they are acting on less than perfect information. That's a well-established tradition in economics, of course. So when you mention rationality, I get a little bit worried about exactly how you define it but both would be

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rational choice models I would argue. One with less than perfect information. The other with perfect information.

Q. Now, if we take a rational choice model but we assume for the moment that people underestimated the adverse health consequences of smoking, in that situation the model would imply that more people would smoke than if they had correctly understood the adverse consequences of smoking. Isn't that so?

A. Yeah, if people had systematically -- have used the wrong probabilities, for example, of risk, then they could easily make a mistake compared to what they would -- at least they would make a different decision compared to what they would have made if they knew the probability, if they knew it to be a different probability or thought it was a different probability, that's correct.

Q. Okay. And would the same thing be true if the mistake by the individual was not with respect to the illnesses that one might sustain by virtue of smoking, but rather was with respect to how difficult it would be to stop smoking once

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addicted to it?

A. That if it were true that agents were uncertain about a piece of the technology, okay, you can adapt those models to account for that. So people would be doing the best choice they could make, but it's a little bit -- that's getting close to this Orphanides and Zervos idea that people don't really know what exactly the full consequence would be.

So they are kind of sampling the good but they don't know they are going to like it as much as they do, for example, and therefore -- and, of course, you know some of the implications that people have suggested, which is that tobacco companies should be -- or somebody I should say should be advertising more than how much -- how pleasurable smoking is just so people won't -- but, no, you are right. There's a sense in which depending on the model, depending on the information structure will get different implications. Yes, for sure.

Q. Now, you used a term or word that's common in the economic literature. I just want to bring it back to --

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A. Yes.

Q. -- the courtroom for lawyers like myself. You talked about agents. The agents we are talking about here are people, society who --

A. Yeah, sorry. Yeah, that's a jargon, piece of jargon, correct.

Q. Every now and then I will attempt to go into the jargon.

A. Okay.

Q. So forgive me if I do so.

A. No, by agents, I mean the individual consumer or family making decisions for the consumers if they are, but in this case I am talking about individuals.

Q. So given all that then, is the following correct? That if people systematically underestimated either the diseases, the extent of illness that they would get from smoking or how difficult it would be to quit smoking once they stopped, then if you corrected their perception of those two issues, fewer people would smoke according to this model?

A. Well, be careful in the sense of saying -- I think I would agree with you in

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general, but I want to make it a very clear statement what I am agreeing to; namely, that if agents or individuals -- sorry -- are mistaken you -- you have to separate out two issues here. That's why I am being -- trying to be clear here, and that is there certainly is a class of models where people have regret that once they undertake something -- they can undertake something, and this can even be true with perfect foresight without any surprise. That people can look at the future one way today and look at it differently tomorrow, and because they are -- and there's no inconsistency because you are talking about their preferences between today and tomorrow today and between tomorrow's today and the next day and those can be different; and the way that one treats the future can be different as one -- as one advances through time.

So there could be regret if that's what you are getting at. There could be regret, and that's been a big literature that people have looked at, for example, in the behavioral literature and that could also be learning which wouldn't be regret, which would literally be the

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sense that you might regret it too, but it's different than the sense of regret. That you might actually learn from the agent -- the agent learns things that we are not aware.

So, for example, I take a totally different example. I'm looking at a job. I sample the job. I work at the firm. I find out it's very unpleasant to be at the firm. So in some sense I didn't make a mistake in advance, but in ex post once I knew how these characters were, what my job would be, I leave the firm. I try another firm. So in that sense there's learning. So I want to separate sort of preference-type regret from learning.

Q. The -- in the regret scenario, would that be a situation where a person at one point in time concludes that he had previously applied the wrong discount rate?

A. That's an example would be it. The regret could -- where you have people applying the wrong discount rate. What they are -- literally what they are doing is they are the discounting the same two dates in the future by different rates at different points in time and, therefore, acting --

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making two different choices.

Q. Okay. For an adult male in the United States, what's the likelihood, the probability that in a given year he will die in an automobile accident?

A. Oh, boy. It's higher than what you would get from a plane accident. I know that, but I don't know the actual number off the top of my head. I mean, I would have to look it up. It's just not something -- it's not -- it's nowhere near 1 percent. But I mean, if I were to put a number, I would probably put it -- I would just be guessing. It's below 1 percent.

MR. WILLIAMS: You don't want him to guess, do you, Joe? What are we doing here?

MR. ANGLAND: No, I will accept --

BY MR. ANGLAND:

Q. You don't have to guess. If you have a judgment, you can give it to me, but I am not asking for --

A. I would say it's certainly -- I would guess it's probably --

MR. WILLIAMS: He doesn't want you to guess.

BY THE WITNESS:

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A. Yeah, I don't know. I suppose as an informed citizen, I should know that, but I just don't. I mean, I would look it up -- it's something I would look up on the web, but I don't -- I don't know that.

BY MR. ANGLAND:

Q. Do you make rational decisions regarding whether to drive?

A. Rational decisions? Try to, yes.

Q. And to make a rational decision regarding whether to drive, do you have to consider the probability of adverse consequences from driving?

A. More the likelihood. I mean, you have to be careful between probability and likelihood.

Q. And how do you define likelihood as opposed to probability?

A. Well, sort of relatively risky situations. So here is my point. I have some perception based on my own experience. I mean, I think many people would sort of round off a probability calculation of this type to kind of the lowest -- you know, based on their previous experience. But by relative likelihood, I mean I

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would imagine situations where I would know the relative risk of damage from driving -- unsafe driving in one situation relative to unsafe driving in another situation.

So, for example, I might avoid certain roads, certain neighborhoods or a construction site where it might be material falling on the road. I would have a pretty good idea of the rough probabilities of that but I wouldn't -- you know, relatively, relative odds. I would say it's relatively more dangerous to drive here than it would be to drive there. So I would know that. I think people would need to know to that to be informed, yes.

Q. So --

A. So if you are asking me -- see, what you are asking though is something interesting because you are saying, well, do I really believe that rationality is tied up with like so-called savage axioms where people would have utilities and probabilities, and there I would have my doubts, you know. I think people have their perceptions of these probabilities. I think a lot of people might round them to zero in some cases.

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We know from some work on behavioral economics and so forth that people sort of take a small probability and sometimes round it to zero or a fairly large probability and set it to one. So we know that there may be some mistakes in that kind of perception. But again, it's a question of how frequently I am driving. If I -- I drive around the City of Chicago. I have a pretty good idea what my chances are but, you know, it's a question do I need -- you know, it's like the old question that was raised by Friedman and Machlup and all these other people do I -- do I actually have to be maximizing my utility function to be a utility maximizer? No, I have a set of rules that sort of emerge that look like I do know the probabilities and I do know -- and the way that actually implements itself is in the form of institutions, my avoidance behavior.

So in other words, that's where we have to be careful about the word rationality. So I can be rational in the sense of following rules of thumb that actually are well-established and could be given a formal justification, but I don't need to. You know, you probably heard of this old

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example that Friedman had or Machlup actually about the pool player who could play pool very well but didn't know the laws of physics. So he could describe the pool game as going very, very well, and yet he was really in some sense hyperrational, but he did it because he was -- he had been shooting the cue and sticking around pool halls and so he gained a certain kind of intuitive knowledge.

So I would guess that people intuitively know those probabilities especially on driving repeated events because, you know, if there was a road, a bridge or something that was going out or something, they would know. It's a rough idea, rough idea. Within a range, yeah.

Q. And if I understood you correctly that even if you don't know or are wrong regarding the actual probability of, say, driving, if you had a sense of the relative danger of driving Route A or Route B, that would be the information that you would use?

A. Well, really sort of the difference, not so much the ratio. The difference in the probability sort of between -- so if I thought Route A would -- would -- you know, if I thought

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like there was a 10 percent higher chance -- see there's a real question as to many -- how -- whether people are actually thinking directly in terms of probabilities in terms of their decision making, and there's some literature on that which asks, you know, do people correctly perceive probability.

My view is that there's a -- that it's the difference in the probability that's really critical, and people I would say grope their way into that, but they do it by experiencing the event, by hearing about the event, you know, social learning I guess. I mean, I would not want to say that most economists -- no economist that I know of says the world consists of people solving, you know, optimization problems at every second of the day with full information probability. So like the Becker-Murphy model you were describing is an idealization of behavior. The only thing that's interesting is that it seems to fit the data pretty well like, you know, as the literature shows.

Q. In general, what has been the trend in smoking over the last 40 or 50 years, current smoking?

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A. Oh, well, we have had a -- well, in the trend generally certainly since the mid-60s has been a decline in the number of people -- well, not the number of people, but the -- but the rates, the fraction of people smoking. Even in the '50s there was a decline in the amount of smoking per person smoking, but there's certainly been a decline in initiation rates say between '63 and '99, for example. The number of people smoking has reduced maybe for -- it depends on whether they are looking at men or woman and it varies across minority groups, but, you know, it has been cut down dramatically, half.

Q. Why has smoking decreased over that period of time?

A. I think a lot of studies -- certainly if I look at the econometric literature, I am more competent to discuss; but just looking generally at the literature, you find that people were aware of the risks of smoking and became aware of the risks of smoking. And when they were acquainted with the structure of a substitute or some kind of lower risk item in their portfolio, like switch to filters, switch to lower tars, they tended to move

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in that direction. So --

Q. I am sorry. I didn't mean to cut you off. I was just going to say for the moment, I want to focus upon the total smoking prevalence that into include people who switched to -- to lower tar cigarettes.

A. Right.

Q. I understand the rest of your answer did deal with that --

A. Right.

Q. -- but just to keep -- keep my question focused upon the overall smoking rate.

Now, you indicate that increased knowledge of the risks of smoking resulted in the decline in smoking prevalence over the last several decades?

A. Well, the evidence seems to be consistent with that is what I am saying. So if I look, for example, at the declines that happen after the 1964 Surgeon General's report or for that matter if I look at the information that seemed to come from the first health scare '53, '54 when you look at the dropping off in smoking among people -- the reduction of smoking among smokers --

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consumption, I should say, among smokers, that what you find is a pattern that seems to be consistent with the fact that people are responding to the information given to them.

Q. You mentioned two specific informational events --

A. Yes.

Q. -- a smoking scare in the '50s, and the Surgeon General's report in 1964?

A. Correct.

Q. Were those the only two events in terms of disclosure of information regarding smoking -- adverse effects of smoking that resulted in the decline of smoking over the last several decades?

A. Well, it depends on what you mean by an event, but I think one major factor -- I don't know if you want to call it an event -- is price. And there have been a series of those events over time where prices have changed partly by government policy and partly for other reasons I suppose. But certainly price has had a huge effect in reducing smoking, and that's pretty well established, and there are a number of events. So states have enacted, for example, legislation jacking up the

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price of cigarettes, and that seems to have a pretty big effect. So many, many events of that sort.

Q. For the moment, I want to confine myself to the reduction in smoking prevalence that is attributable to an increase perception of harms due to smoking.

A. Okay.

Q. And my question is: Whether there is anything other than the 1950s health care and the 1964 Surgeon General's report that over time has increased people's perception of the harms due to smoking?

A. Well, I mean, again when you say --

MR. WILLIAMS: Is your question increase beyond -- since 1964?

MR. ANGLAND: Yes, or I -- let's do it that way. There could be something I suppose between the early '50s and '64, but let's take it since 1964.

BY THE WITNESS:

A. Well, in the sense that, you know, the '64 report was like the first wave of a series of information events that occur. So, for example,

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there are a number of studies and reports that come out in the wake of the '64 report. Studies that are suggesting what might be done, what might be a safe remedy, what might be a good substitute.

So I would say as an information event would be the endorsement of some government agencies of lower tar cigarettes as a means of circumventing the problems arising -- or the risk from smoking. That's an event that starts -- it's not like a red letter day as dramatic as the '64 report, but there is an announcement, there are discussions, panels are meeting saying, you know, lower tars, lower risk in the '60s; and in that sense there are information events of that sort.

You know, Dr. Selikoff's study to go to Falise was an event that people now found that there was information out there regarding the synergy between smoking and asbestos and other synergies that were revealed as various occupational groups got in. So I don't want to say -- I think these are the main events. The '64 report is like to me the tidal wave. It sort of swamps everything else, but there were many other events that occurred during that time and gave

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people information and most of the information, changed the information, suggested hopes, made promises available and so forth.

BY MR. ANGLAND:

Q. In the year 2000, is the public's perception of the health risks of smoking different from what it was in say 1965?

A. I think the literature accumulates. I mean, you have now a whole generation of people -- first of all, you have to understand that the educational level of the whole population has gone up. With the educational level going up, we know even back in '65 that people who were more educated, were more aware of the risks. I mean, that's just a fact. We have now a college level that's -- the percentage of people going to college has increased greatly between '65.

So just on a level of education in the population, there's been a substantial increase which would make people more aware, more acutely aware of -- just more informed I guess is the problem. I also think that we have a number of interventions, a number of studies. It's a topic that has been researched. I have seen estimates of

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somewhere between 70 to 100,000 articles in scholarly and popular journals written on smoking since '65, and that body of knowledge has to accumulate into a body of information about the -- smoking.

So I would guess, yes, it is probably increasing a lot. However, it is still important to recognize that that first wave was like a huge event in the sense that you do see trends in the data that just weren't there, and most of the econometric studies find a very strong effect. Some effect of the '53, '54 period that seems to carry through and the '64 effect that carries through but a trend. Every study also tends to find a trend line of reduced consumption. That's partly I think increasing education, retirement of older people who simply didn't want to hear the news or whatever. So there are a number of factors.

Q. If you go back to say 1970, so a few years after the 1964 Surgeon General's report, --

A. Yes.

Q. -- were are there still some people in this country who did not believe that smoking

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appreciably increased one's risk of adverse health consequences?

A. Well, there are still some. There are people who believe the earth is flat and so forth, but I definitely think that you see a trend. So you go -- the Surgeon General's report in '89 documents this trend that you are getting people going forward into a period that -- you are getting people willing to risk -- talk about what the -- statements like does smoking cause heart disease, does smoking cause lung cancer. You are getting to the 80, 90 percentile figure in the late '80s, and in the '60s -- I mean, late '60s, early '70s it's a lower figure. You get a big jump, but it's not 100 percent.

Q. In the late 1960s -- from the mid to late '60s in response to the Surgeon General's report, did tobacco companies or other entities they control make any public arguments or statements that the 1964 Surgeon General's report was misleading and that it overstated the risk of smoking?

MR. WILLIAMS: Do you want to identify the entities they controlled?

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MR. ANGLAND: Well, let's leave it tobacco company or the Tobacco Institute.

BY THE WITNESS:

A. To my knowledge, there were some statements of that sort.

BY MR. ANGLAND:

Q. Okay. Let's take a snapshot in time in 1970 and let's look at the group of people who did not believe that smoking was very harmful to their health. Okay?

A. Uh-huh.

Q. Is it possible that some of those people would have believed that smoking was harmful to their health had they not heard the tobacco companies' rebuttal of the 1964 Surgeon General's report?

A. You mean is it logically possible?

Q. That's the first question, yes.

A. Yes, it's logically possible.

Q. Do you have any view regarding whether, in fact, that is the case?

A. Yes, I do.

Q. And what is that view?

A. My view is that based again on my

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reading and my -- and again this is partly a matter of looking at the history and looking at the events, the information flood that came after '64; but the structure that I see it is that indeed everybody wasn't fully informed. But there was this flood of information, not just from one agency in the government, but from a number of private organizations, medical organizations, popular journals at all levels aimed at all audiences that made the familiarity with the dangers of smoking something that was out and commonplace. It was topical. It was in the air. It was literally discussed in the media and media of all forms.

So that makes it less likely in my opinion that there would have been any substantial effect above and beyond the large effect, the huge effect that was there in the late '60s and early '70s.

Q. Well, let's -- first of all, can you quantify what you mean by substantial?

A. Well, no. I mean, I don't have an econometric number, a point estimate to get that. What I am suggesting though is that I think -- I don't want to -- since I said I can't quantify it,

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I shouldn't just try to quantify it now. But what I mean is that I believe that if we look at the size of the information flow coming out from all the other sources and the larger society, it seems difficult in my opinion just looking at the evidence, looking at the data that I see, looking at the studies to see that an additional flow of information especially from an industry that has sort of a -- you know, just one industry would have a sizable effect.

Q. Well, I was ask going to ask you -- strike that.

Let's pursue this a little further. Let's assume that John Doe is somebody in 1970, and he has heard about the Surgeon General's report. He hasn't read it, most people don't read it, but he reads the newspaper or he hears stuff on television.

A. Right.

Q. But he also hears the tobacco company saying all of that is wrong. It's just statistics, and you can play with statistics in a whole bunch of different ways, and you are not really comparing apples to apples in those studies, and there's no

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real evidence statistically. When you realize all the statistical limitations of the studies, there's nothing that actually shows that smoking causes adverse health consequences, and we have got experts to back those up, just like those other guys had experts to back them up.

Let's assume that Joe Doe has heard both the Surgeon General's side of the story and that side of the story, and rather than speaking about one John Doe, we will speak about a million Joe Does who have heard both those stories. And let's assume that some of that million of people wind up -- is that possible that some of those millions of people -- that million people would wind up concluding that -- concluding that now probably cigarette smoking isn't all that harmful?

MR. WILLIAMS: Are you done?

MR. ANGLAND: Yeah.

MR. WILLIAMS: That was quite a mouthful. Let me object to the form of the question.

BY MR. ANGLAND:

Q. You can answer.

THE WITNESS: Should I answer it then?

MR. WILLIAMS: If you understood it.

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BY THE WITNESS:

A. I understand it sort of. When you say logically possible, yes. I mean, lots of things are logically possible. Let me though just add. In your question you see there are several issues there which aren't as strange and fraudulent sounding as -- as you could interpret them to be in the sense that the Surgeon General's report, as you know I am sure, was a landmark in the sense that it really caused, it founded the subject of modern epidemiology, which is a very interesting subject very, and there was a concept of what causality meant which was designed in a laboratory, controlled in a laboratory setting. That was a fundamental piece of traditional epidemiological, certainly biological science lore, sort of called Karush postulates or whatever were basically ones of experimental variation. And just looking at -- this is something I am very interested in my own work.

So you see a huge sea of change by that Surgeon General's report. Now, a committee comes out and says that, but if I look at the literature in econometrics, actually this report had a huge

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impact indirectly across a whole broad band of social statistics. It gave rise to the question of is there a statistical basis for establishing causality. So I am saying all that in a long-winded way just to -- maybe it's a long-winded response to a long question.

BY MR. ANGLAND:

Q. I deserve it.

A. No, but what I am saying is that part of what you are saying is that there would have been -- I don't want to say, you know, in 2000 that there aren't -- you know, I don't want to start making a lot of strong statements here, but I do think that there was a very non-trivial question which continues to this day about how you establish causality from observational studies which is what the '64 report did.

And so many statisticians -- in fact, if look at the Journal of the American Statistical Association last March, a special issue, a very famous guy writes an article, a good guy actually, a guy who is actually named H.D. -- H.B. or H.W Dawid, D-A-W-I-D, in the March 2000 JASA essentially arguing you can't make causal

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statements from observational planets. The whole idea of a causal model -- so all I am saying is that from a purely logical viewpoint since we are dealing with logical possibility, it's an open queation about what causality -- and in particular it was a lot of unfamiliarity with the statistical association.

So I am saying that the statement is not as, you know, patently fraudulent certainly in the 1960s as it might sound now because we have accepted it. Having said that though, I accept the logical possibility that if a person was on the margin and he had this and this and if that person hypothetically weighted these two sources equally, then it could well make a difference, you know, in the decision of that person to smoke. I have no doubt that is true. I think that you would have to say that.

The question is what weight the individual would place on the information coming from the difference sources, you know, established panels of experts, HEW secretaries, members of the Readers Digest, Life Magazine or even religious organizations that got into this business. How

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much they would place that versus the tobacco companies which at that point would then be viewed as saying, well, they have a vested interest to deny this.

So to me, again, this is a question of judgment, not of an econometric estimate. I find it possible, but I don't find it to be a compelling and major component that would be seem to be offsetting the magnitude of the information provided by the Federal government and other agencies.

Q. At the end of your answer, you used the words compelling and major again and --

A. Yes.

Q. -- I want to try to turn those into numbers.

A. Numbers. Okay.

MR. WILLIAMS: What is the question?

BY MR. ANGLAND:

Q. Let me ask you a question.

You said that as a matter of judgment you didn't think that removing those statements that I referred to as at least hypothetically having been made by the tobacco companies would not

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have had a major affect --

A. Right.

Q. -- on smoking prevalence. What do you mean by major?

A. Well, like reversing, for example. To me, it would be implausible for one reason alone. I don't know if even the tobacco companies would have the resources to offset the mass amount of publicity given to the Surgeon General's report and all the ensuing discussion both in scientific journals and in certain popular press, but even if they had, I guess at that point they were no longer viewed as an impartial party.

The Surgeon General was not viewed as somehow selling a product. The Surgeon General was trying to endorse -- presumably to promote the health of the American people. The tobacco companies I assume -- I think it would be reasonable to assume is what I am saying by my question would be in a position where they were defending themselves against a serious charge, and therefore is suspect in the sense of denying something that had been established by an independent government body.

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So in that sense, I think the credibility would be lower. You know, when people are sort of looking at risks typically, you know, you look through authorities; but you also would look at those who were sort of maybe giving you the higher of the two risks. So I think that's the sense in which I am saying, but I don't have a precise econometric estimate.

Q. Let's assume that over a given period of time smoking prevalence declined from say 35 percent to 30 percent in the real world.

A. Uh-huh.

Q. Presumably there was some point of time over which that was true?

A. Yes, that's true.

Q. Assume further that during that period of time while people had heard the Surgeon General's story about the risks of smoking, they had also heard the tobacco companies' rebuttal of the Surgeon General's report.

A. Uh-huh.

Q. Now, let's make a counterfactual world where those same people -- where the population heard only the Surgeon General's side of the story

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and did not hear the rebuttal by the tobacco companies.

Do you have an expert opinion regarding whether the -- at the end of the period in that counterfactual world, smoking prevalence would have fallen to 25 percent as opposed to 30 percent from the 35 percent starting point?

A. That's a very -- let me put it this way. I know of no study that would allow anyone to proceed with the precision that is required to answer your question. I have not read a study that would do that. That's a very demanding task.

So the answer is I can't answer the question. I can't answer the question with any precision. It's -- what I am giving you is an intuition and a weight and also drawing on a whole series of other studies where information events have been given to people in the wake, you know, like the interventions of the '70s and '80s where people were actually actively engaged in informing people about the health risks so more than just providing a statement, and these people don't take that information. They don't seem to act on that information. That's as if they know the

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information is there.

So somehow I think of information as a stock. It's built up. You get this huge flow from '64. It's elaborated on. The major events occur. At that point it is not erased, but then when we try to add to it, it doesn't seem to add too much; and you are kind of going in the other direction in your hypothetical saying if we -- if these companies tried to subtract from it, would they have much of an effect.

And again, it's my judgment not based on econometric model though that there is a -- that it's implausible for there to have been a large effect, and I -- again I realize you are going to ask large effect. So I mean, but I think that undoing what was there and established in the larger network of information flow would be extremely difficult to dislodge.

Q. Just to make sure I understand, is it -- with respect to -- it's your position that you really can't answer, you don't have the information to answer whether the removal of the statements by the tobacco companies would have caused prevalence to drop from 35 percent to 25 percent during the

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period when in the real world, it fell from 35 percent to 30 percent?

MR. WILLIAMS: Joe, I object to the form. That's not what he said.

BY MR. ANGLAND:

Q. You can answer.

A. Well, let me -- let me -- let me reformulate --

Q. Okay.

A. -- if I may, and that is if we look at the econometric estimates, we find that there's a substantial trend line that seems to start in the '50s and certainly accelerates it seems in the post-60 period. Again, you know, these are time series evidence. That's strongly indicative of a very big trend put in place. Okay. That I think we accept, at least I accept from finding the literature in the literature and literature surveys seems to find that those informational events had this large effect.

Now, we are asking -- now, we are going way outside the data and saying can we now conjecture what would have been the world like if we had somehow done this thing or that, but we try

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to make this conjecture without any information, without any -- without sort of an econometric model. I mean, this is sort of the essence of what -- so you are correctly looking at the right point, but then the essence of it says, well, I turn it around and say, well, what is the evidence that shows that there would be this kind of powerful ability to reverse something as well-established as the cigarette smoking causal relationship.

My own judgment suggests given the weight of the substantial body of evidence that it would be difficult to reverse that, and that the activities that I have seen or understand the tobacco companies undertook would be of limited value in reversing that trend, but there's no econometric evidence. So it's a -- on both sides. That's the crucial thing.

So this is a matter of pure conjecture saying that we can somehow imagine -- so I mean, I accept your question as a conjectural question, and I am saying I think I can know in principle how to go about addressing it. I mean, the obvious thing would be to look at studies across different

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countries or something where sometimes industry had been more cooperative than other countries, but there are no such studies to my knowledge. And so at this point, it's pure speculation, and that's why -- but having said that, I also think that the weight -- and here is where one is judgmental. It's not coming out of a number, nor does the -- nor does the other notion that somehow a conspiracy slowed this down come out of a number.

The point is at this point we enter the realm of conjecture, fantasy or counterfactuals without a basis or free floating. That's where I worry. So I suppose it is logically possible it could have fallen ten points instead of the five points in your scenario, but I find very weak evidence that would support such a claim or that the activities had any -- of the tobacco companies had an effect.

If I sort of look at events and look at the time series of it to say that the tobacco companies issued this report or that report, I don't find any kind of decline in the quitting, for example, associated with that which I would expect to find or denials. And so that may be because the

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time series is very limited and it's highly aggregative, but to me it suggests there isn't a very big smoking gun buried there when I look at the time series. I mean, if you look anecdotally at the time series and reanalyze the data for periods of time, you know, denials and so forth that are issued a few years later or some report, it's very difficult to find that.

Q. I am sorry. For that purpose, do you look at the denials as a stock or as a flow?

A. Oh, in both cases you would want to look at the denials and for that matter the information as a stock, correct. I mean, and you are absolutely right. In this literature one of the biggest problems has been taking stock-type events and looking at flow. Many people looking at the event studies though would try to find some change in the rate of direction. So the flow would have an effect on the rate of change. You know, you are just differencing it out. So in that sense it would be, you know, going from flow to -- stock to flow and then flow to change. And that's the right dimension. That would keep it in the same.

Q. With a slight variation of the

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hypothetical I gave you early -- earlier. Let's assume in the real world over a period of time smoking prevalence fell from 35 percent to 30 percent.

In our counterfactual world the Surgeon General's statements stay there but the tobacco companies' rebuttals go away, and the question now is -- so it's the same set up as the prior question.

A. Right.

Q. The question is: Do you have a view regarding whether in the counterfactual world smoking prevalence would have been as low as 28 percent at the end of that period instead of the 30 percent that it was in the actual world?

A. That's a small permutation of the previous question.

Q. It was intended to be.

A. No, and for the same reason I would give -- I would just regurgitate the question in response to the previous question and say that if we were to look at sort of a precisely formulated econometric model, I know of no such estimates that would give it. My -- however, my judgment is that

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it's not going to be very important. 28 percent versus 26 percent, no, or 25 percent, I can't --

Q. I am sorry. I lost you with the numbers at the end. Let me just state the question again.

A. Okay.

Q. And I understand all the points you made about there not being a precise econometric study that addresses it.

I am just saying given all that, do you have a judgment regarding whether smoking if prevalence in the real world fell from 35 percent to 30 percent during a given period, whether in the -- if in the counterfactual world there were no rebuttals by the tobacco companies, smoking prevalence could have fallen from 35 percent to 28 percent; that is, two percentage points more than in the real world?

A. But what I am saying -- and again in response to the question. So I think it is the same in the re-answer which is that I know of no study, no sort of rigorously formulated study, empirically-based study that would suggest that claim could be substantiated.

Q. Do you know of any that suggests that

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that -- that the claim is wrong?

A. Neither right nor wrong. It's fantasy. It's speculation.

Q. Okay.

A. My own view however is based on the weight of the information out there, that -- that seems implausible, but that's different from saying I have a number that I can come out from a computer and produce and say -- nor has anybody on either side of this case as I understand really provided that kind of number which is the essence of constructing a good counterfactual, sort of anchoring it in data.

Q. Given the data that exists in the real world --

A. Yes.

Q. -- do you know of an econometric way to answer that question?

A. Oh, yes.

MR. WILLIAMS: Which question are we on now?

BY THE WITNESS:

A. No, this is the question about reduction of --

BY MR. ANGLAND:

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Q. That one.

A. I guess he should answer the question, but basically just reduction of information. So sort of removing the opposition.

Well, I will tell you the way that most people would start to answer the question, and that would be to try to build a stock of denial capital, if you will. A stock of sort of press releases, advertising reports, statements that were made just like you would build a stock of advertising capital and number of, you know, messages that Philip Morris or any other company would bombard the audience of potential consumers with.

You would also have a similar kind of media stock where you would cumulate it and put it into a regression and then ask, well, do we find any evidence that this stock has changed. That's why I mentioned later in my -- in response to the question -- earlier question, later part of my answer, that, you know, one way would be possibly to do an event study but look at rates of change to see, well, those stocks may be difficult to measure, but we can measure rates of change through events. I mean, they look at change in initiation

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rates.

I don't believe it's intractable to do that, and to be honest with you, one of the things I did do, again this is not in a report, but if you just look at the events, history, just look at the date and the prevalence rates and you introduce these events into the time series, I find no effect. That's not in this report. And so I am coming way out of left field. We talked about what I had done.

So I think it is logically possible to do, and, in fact, it's empirically possible to do that. And when I do that, I find no effect; but on the other hand, that's -- I didn't enter it precisely because I think it's open to criticism; and the criticism is simply, well, you know, these are -- this is a small part of a large time series and, therefore, it's diluted in other events; but I can't find the smoking gun.

Whereas I can find a smoking gun in '64. I can find a smoking gun in '53, and so if there's a kind of smoking gun out there and there are these big denial events out there, you could expect to see something in the data, and I just don't. Could

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be absorbed by other thing though. So in fairness to any position, it could just be something bigger that's out there.

But then there's a notion of sort of threshold and noise, and whatever it is it seems like this kind -- these kinds of announcements seem to be more like noise than sort of big stuff about the threshold. Whereas '64 is like -- is like Moby Dick rising above the ocean floor and showing itself in the time series. So you don't have to speculate that hard in the time series to find '53, '64 events.

This stuff is very problematic. Now, people have tried to do this. Like, for example, another totally different episode is the fairness doctrine business where there's been a lot of discussion; and as you know, I suspect there's a lot of contrary debate in the literature pro and con about whether or not the fairness doctrine which was another kind of intervention shows up. All I am saying is that this is below the threshold of detection even of the fairness doctrine which itself is controversial. So that's why I say it's not that big. I don't think it's that big, but

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it's again saying it's logically possibly as big. Some mechanism that I haven't investigated.

Q. Let me ask you to -- ask the reporter to mark as Heckman Exhibit 1 a document entitled, Expert's Disclosure by James J. Heckman, January 17, 2000.

THE WITNESS: Okay. I have it here.

MR. WILLIAMS: Do you really want to mark it? Why do we need to mark it? Everybody has a copy.

MR. ANGLAND: That's fine. Normally, it's defense counsel who wants it marked, but I am willing to --

THE WITNESS: You have it, okay. Fine.

MR. ANGLAND: -- willing to go with it and make this lighter for all of us carry when we load the Heckman exhibits.

BY MR. ANGLAND:

Q. Okay. Can you identify what would have been marked as -- I can see why people mark these things.

A. Yes.

Q. Can you identify the document that I will just refer to -- well, can you identify the document in front of you?

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A. Yes, I will hold it up to the camera. This is the document, correct.

Q. What is it?

A. It's a report that I filed, Expert's Disclosure January 17, 2000, Blue Cross/Blue Shield of New Jersey Incorporated versus Philip Morris Incorporated, et al.

Q. And when I refer to your report for the remainder of this deposition, can everybody agree that I am talking about the report you have in front of you right now?

A. Correct, yes.

Q. Okay. Does this report correctly reflect the opinions and conclusions as to which you will testify in this litigation?

A. Yes, although I guess I would want to keep a caveat open that if I find something else, I might want to -- but this is the report that has been filed and this is the thrust of my testimony.

Q. Were there any corrections that you wanted to make in this report?

A. Not corrections, no, but there are these other reports that I already mentioned, and --

MR. WILLIAMS: We also updated the Reliance

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list I believe.

BY THE WITNESS:

A. Yeah, we added a couple of -- do you have the updated list here?

BY MR. ANGLAND:

Q. I am told while I was out in Detroit the last couple of days, there was a December 2000 article --

A. Yes.

Q. -- that reflected an update on the Reliance list. Was there something else?

A. Mulholland I think, right, on advertising. Mulholland, an FTC report by --

MR. WILLIAMS: I think that was identified before.

THE WITNESS: Oh, was it? I thought it wasn't.

BY MR. ANGLAND:

Q. Okay.

MR. WILLIAMS: But at any rate, that is part of his Reliance, the Mulholland report and this most recent study.

MR. ANGLAND: And we received a letter on the Mulholland report?

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MR. WILLIAMS: Yep.

MR. ANGLAND: Okay.

MR. WILLIAMS: I am not sure that was subject of a letter. That has been identified before.

MR. ANGLAND: Okay. That's all --

MR. WILLIAMS: This new report is the subject of a letter that you should have been received very recently.

MR. ANGLAND: Okay.

BY MR. ANGLAND:

Q. Do you have any ongoing projects in terms of new analytical work relating to this litigation?

A. Well, only in the sense -- in related litigation in the sense that the National Asbestos Workers study, and when I was mentioning in my prior remark about looking at interventions -- looking at disinformation interventions, I did this anecdotally in the sense of just looking at the time series and putting in dummies for events, and just nothing was there, nothing jumped out. That's it.

Q. And that was in the National Asbestos you introduced dummies?

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A. Yes -- well, no, no. That's not even filed in the report. That's just like background work. This is a case where I wasn't trying to annoy the readers by putting in extraneous variables. So the disinformation-type dummy variables that we tried to identify from various episodes in reports and so forth. For example, in Falise it turned out that there was a report that the Tobacco Institute I believe issued in '79 tried to rebut some remarks of the Surgeon General.

So we tried to see, you know, what's going on, was there any kind of effect, and it was just too small to detect. So much so that we just didn't introduce it as evidence. We thought it wouldn't be taken seriously.

Q. And just to perhaps shorten this line of inquiry, for the purposes of this litigation, you are not relying upon that empirical work, are you?

A. No. No, I guess I can -- should I reserve the right to use everything?

Q. I guess everybody will reserve the right to object if you try to.

A. Yes. No, but that's not my intention based on this report. It's the use of this data.

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Q. All right. In paragraph 6 of your report on page 3 you say among other things that plaintiffs quote "misapply economic theory" close quote?

A. Yes.

Q. What -- and is it fair to say that paragraph 6 is part of sort of a summary, an introduction and you give more detailed analysis later on?

A. Correct, that's correct.

Q. Which portion of the remainder of your report do you believe falls in that category; that is, misapply economic theory in distinction to misinterpreting economic data?

A. Oh, well, here is the -- if you were to go back in the report, for example. Let me just look at this for a second here. For example, if we were to look at the study -- I am just trying to find. For example -- oh, here we are. The map, for example, in paragraph 44 when Dr. Harris is talking about the 4.5 percent rate in health hazards and innovations. I just have to look at this where they say it.

Because there there's a map of saying,

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well, we go from -- for example, this is just one example -- where we go from the statement about what R&D rates should have been in the industry to what sort of -- the rates have declined and then what this factor that was identified as R, relative risk factor, how that would have declined. I think that's a misapplication of economic theory.

It's going from a -- it's going from a statement about R&D input without going through an economic model or mechanism which shows how that maps directly into morality or to health expenditures. So that's an instance. I also think there's a question about the prevalence -- the economic model of smoking behavior and sort of what you would expect to have sort of large changes and small changes and what a rational addiction model would show. So when I say misapply, I think really there's a sense in which the model of decision making of the consumer isn't well stated, and then there's a model of the decision making of industry which is not clearly articulated and in that sense misapplied.

Q. In --

A. I can be more specific if you want. I

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mean, the two are related actually, the application of the data and the actual model or even the simplest idea. For example, in fact, just looking at paragraph 48, the idea that if you make a risk -- if you eliminate a risk that you get -- it's a basic point we teach all of our students everywhere and that is, if you make a risk less risky, people may engage in more of it. So you may end up with more of the risky behavior than less of it.

So in that sense it's a fundamental -- I think it's a misapplication to somehow say that reducing the risk, would have reduced the behavior. It could have promoted the behavior. So there are a series of cases throughout the whole document that have this character. I mean, if you want me to go through it page by page, I could.

Q. No, I was really trying to understand what you meant in the sense by misapplying economic theory, and I think the examples that you have given --

A. Those are some of the examples, but also the question of what industry behavior should be and what collusion should be, what a collusive

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response should be. How a monopolist or an oligopolist should respond to certain kinds of information interventions and so forth and so on.

And then also the -- a fundamental -- a fundamental distinction I think would be the notion between saying I am not sure you could call this economics, but the difference between leading a horse to water and making him drink which is really a fundamental piece of this which is saying that making a good available is not the same as making -- having the consumer buy the good, and that's I think the major difference. And I consider that an analytical error to say if I make -- if I make the Edsel available in 1959, everybody should have been buying the Edsel. I guess it was '58 with the Edsel but whatever. That is I think -- I would call that a misapplication of economic theory because there's nothing in economics that says that people buy a bad good.

Q. Let's go back to your comment about oligopoly and monopoly.

A. Yes.

Q. In which respect do you think what Dr. Harris did was inconsistent with proper

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economic theory regarding monopoly or oligopoly?

A. Well, proper economic theory it's just there's an implicit theory of how a monopolist or how an oligopolist would respond, for example, in the situation of colluding. So there's a notion, for example, that there's collusion among the industry in the late '50s, and the argument is that collusion in the industry should take the form of not competing over health. There was some discussion. You don't want to compete over health, and so this is an implicit premise on which we complete.

Now, on the other hand -- so I don't know if this is going to verge from a theoretical point to an empirical point, but the empirical fact is there seems to be some competition over tar, some competition over filters, and that sort of thing. And it's not obvious that even in -- so I guess, they are saying from the a priori logic of what an oligopolistic industry would do it's not obvious that a priori that they would form this as their principal vehicle of coordination activities.

You know, there's a richer theory of monopoly which you could have imagined which is

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more along the lines of George Stigler saying, well, here you have a regulated monopolist under public scrutiny. You might think they would try to capture the regulatory agencies and they would even try to administer price changes, information changes, even advertising regulation to make their own benefits. So there's a theory of collusion you could say that would come from why you might want to shut down advertising to keep incumbents in place.

I would work that out in a somewhat more rigorous way than was worked out in the report. This kind implicit behavior of how a collusive industry operates, and I don't think it's -- so misapplication may be a little strong, but I think in some details I think it is like the business about making goods available.

Q. Is there anything in the Stiglerian theory of oligopoly as reflected in his article back in the 1960s that suggests that this type of conspiracy that Dr. Harris posits was not plausible?

A. It would take a different form I guess in the sense that if you thought that -- if you

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though an industry as being regulated as it is, you know, now in the '60s when it starts to be regulated, that's when Stigler's theory about the regulators are taking over by the regulated, if that's are what you are talking about. Stigler had a bunch of theories. That's why I want to be careful.

Q. Yeah, that's fair. Are you familiar with Stigler's article it may have been in '63 which talked about the industry characteristics that facilitate effective collusive behavior?

A. I am familiar with a big body of Stigler's work. The '63 paper I read it years ago. I don't quite remember the details of it, but I think I know the general argument, yes.

Q. Is there anything in that Stiglerian view about the conditions that render an industry susceptible to oligopoly -- oligopolistic and collusive behavior -- let me back up. I mixed two concepts there, structure and behavior.

A. Yes.

Q. Is there anything in Stigler's analysis regarding the sort of industry characteristics that facilitate or make possible collusive behavior that

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would suggest that the behavior that Dr. Harris posits was not possible?

A. No.

MR. GARNICK: Object to the form.

THE VIDEOGRAPHER: Counsel, I need to switch tapes. Going off the video record at 11:04 a.m. at the end of tape 1.

(WHEREUPON, a recess was had.)

THE VIDEOGRAPHER: Going back on the video record at 11:14 a.m. at the beginning of tape number 2.

BY MR. ANGLAND:

Q. Dr. Heckman, back on page 3 of your report in the first sentence of paragraph 8, you indicate that contemporaneous studies -- surveys show that the public understood the health hazards associated with smoking.

Do you see that?

A. Yes.

Q. All right. Let's take a look at some point of time in say the 1970s, and is it your view that at that time the public understood the health hazards associated with smoking?

A. Well, if you look at the questions, for

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example, the time line of the questions that are reported say in the Surgeon General '89 report or many of the Surgeon General's report, but '89 was kind of like a summary, 25 years, you rely on that to the '70s, you find that people are aware of -- in a way they weren't in the '50s of the risks associated with smoking. So, for example, lung cancer, heart disease and the like.

Q. And so is your answer then, yes, that if you pick a point in the mid-70s, the public did understand the health hazards associated with smoking?

MR. WILLIAMS: Well, his answer was his answer.

MR. ANGLAND: Well, okay. Then I will move to strike as nonresponsive to my question and ask the question again. Don't like to do it the formal way, but I will do that.

BY MR. ANGLAND:

Q. I am not asking what your source of evidence is. My question is: Is it your opinion that in 1975 the public understood the health hazards associated with smoking?

A. By the public, if you say do the

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majority of people understand that there are risks associated with health, the answer is the majority of people at that point just overall population now totals, we could look at subpopulations, were acknowledging substantial causal relationships between smoking and other medical conditions. And so I would say in that sense the answer is yes.

Q. Okay.

A. But to know the exact probability distributions, no. Like we were saying earlier, nobody knows those probably ever.

Q. Let's -- let's examine seven dimensions of that public understanding. No. 1, at this let's say 1975, did 100 percent of the people of the American population believe that smoking had a substantial adverse impact on health?

A. No, I am sure they didn't. Many of the surveys show it's much less than --

Q. Okay.

A. I mean, there were people who thought there was no effect. A diminishing fraction but over time 1970, no.

Q. If at this point in time through some change in the mix of information available to the

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American population a higher percentage of the people wound up believing that smoking had serious adverse health consequences, would that in your view likely have decreased the percentage of the population that smokes?

A. I don't doubt that there's any relationship. I mean, I doubt -- I say I don't doubt that there's a relationship between a knowledge or a perception of a risk and response to it.

Q. Okay. No. 2, let's take a look at the subset of the population who did believe that there was -- that there were adverse health consequences associated with smoking.

A. Yes.

Q. Not every one in that group would be 100 percent certain of that conclusion; is that right?

A. I am not sure what you mean by 100 percent certain. They would offer it as an opinion that it would cause risk, but they wouldn't usually give a probability judgment about it. So in that sense it's usually binary, yes or no, it's risky or not. But they may have it in the back of their minds, but no survey that I have seen gave them

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probability distributions.

Q. My question doesn't relate to whether a survey captured the number. Let me just try to put it this way. Let's assume that I were somebody who was surveyed in 1975, and I was asked does smoking recreate substantial health risks. And in my mind I said, you know, the Surgeon General thinks so, but there are these arguments about causality. So maybe the Surgeon General is not right, but I think he is probably is. You know, sort of three to one the Surgeon General is right, and I then answer a question, yes, when asked if I think there are substantial adverse health risks.

Do you have that example in mind?

A. I understand what you are saying, yeah.

Q. In that case I would have my -- my hypothesis a 75 percent belief that -- since I said three to one odds, that there were substantial adverse health effects, right?

A. Right.

Q. Okay. And under the theory of rational addiction, I might or might not wind up smoking given that belief depending upon a number of other variables, correct?

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A. Well, that by itself is not enough to determine whether you would smoke.

Q. Exactly.

A. Yes.

Q. And that if there were a large number of people in exactly my position, some of them would smoke and some of them wouldn't smoke depending on the distribution of other variables?

A. Yes. Correct, yes.

Q. Now, if instead of having a 75 percent belief, I had a 95 percent belief and so did everybody else in this group of similarly situated people, then under the theory of rational addiction a smaller percentage would wind up smoking; isn't that right?

A. To the extent that everything else is the same --

Q. Yes.

A. -- but you just moved from a .75 to a .95 in that probability, then what you are really saying, as I understand it, is that these people would be weighting, doing a utility calculation where they weight their .95 versus .05 and in looking at the utility of the two states versus

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.75. And so it would have shifted in a way towards making fewer people smoke in your example.

Q. That's correct, right?

A. Hypothetically, yes, correct. That's for sure.

Q. Bear with me a moment. I want to simplify this.

MR. WILLIAMS: You have hit two of the seven aspects.

MR. ANGLAND: Yeah. I am deciding the sequence for the remainder.

MR. GARNICK: I was hoping he said several not seven.

MR. WILLIAMS: No, it was seven, and that's how she got it.

MR. ANGLAND: I may condense to six.

THE WITNESS: Seven sounds formidable actually. Seven paths to insight or revelation.

BY MR. ANGLAND:

Q. No. 3, again looking at the people who do believe that there are serious adverse health consequences resulting from smoking, those people might have differing views regarding how severe the consequences are. And to be more specific, I am

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talking now not about the probability of having an adverse consequence, but whether it would be, for example, death as opposed to simply -- I don't want to say simply to minimize it -- but trouble breathing in one's later years but not causing death.

A. Right.

Q. You could have variations in the perceived worst case?

A. Absolutely, correct.

Q. And again under the theory of rational addiction if a different mix of information led people to conclude that the worse case was worse than they thought, it would also follow that for the population as a whole, you would have the smaller percentage of people who smoked?

A. That's correct.

Q. Okay. Fourth, let's look at the portion of the population who believes that smoking can cause adverse health consequences and that those consequences include something like lung cancer that can lead to death, but let's assume -- strike let's assume.

Let me ask you if it's possible that

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within that subset of people, the group could assign different probabilities to contracting lung cancer as a result of smoking?

A. Oh, surely. There could be heterogeneity. People would differ so their expectations or the probability I should say of various events that might occur from smoking.

Q. And for those people -- for that group of people who do believe that smoking can cause lung cancer if as a result of a different mix of information they assigned a higher probability to contracting lung cancer from smoking than they did in the actual world given the mix of information available to them then, that could also lead to decreased smoking under the rational addiction model?

A. For sure. Although again the question is the size of the effect, but for sure.

Q. We will come back to size in a while.

A. Right.

Q. Fifth, in this set of people who believe that smoking can be harmful and recognize that it could cause say lung cancer which would result in death and assign a very high probability to smoking

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leading to lung cancer and causing death -- strike that. I am going to start that one again. I put too many conditionals in. Trying to keep seven things separate is more than I am up to on a Saturday morning.

A. Have some of the coffee here. It's industrial strength.

Q. Again, looking at the group of people in the population who believe that smoking can cause adverse health consequences including fatal adverse health consequences, and -- there could be variation among that group regarding how many different fatal diseases they believe smoking could cause; is that right?

A. Oh, surely. It could be.

Q. So some people might think, yeah, smoking causes lung cancer.

A. Yes.

Q. And that's very dangerous, but they might not believe it caused any other fatal disease?

A. Correct.

Q. If those people for a change in the mix of information concluded that smoking not only

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caused the lung cancer that they were aware of but also created substantial chances of other deadly diseases that could, holding everything else constant, reduce the percentage of the population that wound up smoking; is that correct?

A. It could for sure.

Q. Sixth, -- we are getting there -- for the group of people who believe that smoking caused or could cause severe adverse health consequences including death, there could be variation within that group, could there not, regarding the perception of how long one had to smoke in order to increase one's risk of those adverse consequences?

A. Correct.

Q. So you could have some people, for example, who fully believe that smoking was very dangerous but didn't think it would be if you stopped within say 10 or 15 years?

A. Correct.

Q. And if those people through a change in the mix of information learned that the substantial adverse health consequences could occur earlier than they had previously thought they could have occurred, that could reduce the extent of smoking

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in the population, correct?

A. That's correct.

MR. WILLIAMS: I think I know the seventh.

MR. ANGLAND: I will take that as a compliment because it suggests a certain at least basic logic to the progression.

BY THE WITNESS:

A. And this is a seventh seal, the monster will spring out here. Okay.

BY MR. ANGLAND:

Q. Seventh, within this group -- within the population that believe that smoking could have adverse effects, people could have varying degrees regarding how difficult it would be to stop smoking after they started; is that correct?

A. Perceptions about their own difficulty of cessation?

Q. Yes.

A. Yes. Oh, there surely must be heterogeneity in that regard.

Q. And if, in fact, for a change in the mix of available information people realized it would be more difficult for them to stop smoking than in the real world they thought it would be, could

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that -- would that holding everything else constant under the rational addiction model lead to a smaller percentage of the population that wound up smoking?

A. Yes. I mean, you would think so, yes.

Q. Okay.

A. The only reason why I am hesitating is that you see if you give individuals some uncertainty about the pleasure, that by itself could stimulate a demand, and that's the only thing I am worried about. So you say it's difficult to stop, but on the other hand, if I just added a little bit to your question and said, well, I am uncertain about the benefits, how much do I really like it, then I could actually have people smoking more, right.

So I mean, there's a sense in which I have some uncertainty. That source of uncertainty if people are just searching to try to find out information about their lives or just the world around them, they might engage more, you know, even though they -- so it's just adding to your question. I guess it's eight, and it's not even my place. So I withdraw the remark.

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Q. On that last point though, let's distinguish -- see if we can distinguish at all smoking on the one hand from listening to classical music on the other, a non-randomly chosen example.

A. From footnote 2.

Q. Exactly. Now, I think if I understood your report correctly, you say that listening to classical music could be an addictive behavior because of the temporal adjacent complementarity?

A. Correct, among other things. I mean, it has some of the same -- could have some of the other some problems of addiction too, but yes.

Q. Now, let's -- I will try to just work backwards from the jargon.

A. Yeah.

Q. That -- is it fair to say that in layman's terms a reason that you say that listening to classical music could be addictive is that -- in the economic sense is that when people listen to classical music, they get to like it more and more. The more you listen, the more pleasurable it becomes, and so having listened to it some makes you enjoy it more in the future?

A. It could.

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Q. Okay.

A. It could. It could also make you -- it's not guaranteed. I mean, this are -- but, yes, that would be one reason why it could be addictive.

Q. Okay. And just to be clear, I am accepting sort of that hypothetical --

A. Right. Okay.

Q. I don't -- my wife is a classical singer, but I do not purport to be an expert on this area, but let's just take that as a hypothesis.

In that case looking at the situation at the outset if a person had perfect foresight, listening to classical music he could say, well, I will like it a certain amount in period one, and then I will like it even more in period two, right?

A. Oh, yes.

Q. And compared to his baseline case of not listening to it at all, both period one and period two have higher -- are better periods for him. He has a higher utility?

A. Correct.

Q. Okay. And in non-economic terms, he enjoys himself more in both periods one and periods

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two than he did in period zero, holding everything else constant.

A. Correct.

Q. Now, let's think about the smoking example. Let's say period zero is where the person is not a smoker. Period one is where the person does some smoking, and in period two a person decides to give up smoking.

In that situation, how would his utility in period two compare to his utility in period zero?

A. Could be lower.

Q. Okay. In the case of classical music, and your -- I will just call it your hypothetical --

A. Be careful now. Remember the -- can I just qualify that?

Q. Sure.

A. Because when we are making per period returns, I am answering your question to say the utility if I added up zero, one, two, three or however many periods zero, one, two in your example. So I could be lower in the second period than I was in the zero period, but if I add zero

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one and two together, I am better off. That's the sense, right?

Q. Uh-huh.

A. So in some sense the consumer can still be better off, but that's distributed. You know, the classical example is a case -- the way you set it up where the person is sort of better in one than zero and better in two than one. There's a stream that goes up.

Nonetheless, the thrill of period one in the smoking example could make the guy very happy if you compensate him for the losses in two that put him below where he was in zero. That's the essence of the theory.

Q. Okay. Using that framework, let me ask you whether there is -- whether each of the two following scenarios is possible: We have -- in both cases we have a baseline case of where the person is not doing the activity.

A. Correct, yes.

Q. Then we have period one where he does the activity and shows positive utility.

A. Yes.

Q. But these two scenarios differ in

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scenario two that in one -- in one of the two scenarios in period two when he stops doing the activity, his utility falls back down to the baseline level.

A. Correct.

Q. Whereas in the other -- other scenario, it falls down to below the baseline activity.

A. That's possible, yes.

Q. Both of those are possible; is that right?

A. Yes.

Q. Do you have a view regarding whether smoking falls into either of those scenarios?

A. Well, I think from what we -- first of all, if you were to actually estimate the models of utility -- a new model I just saw from one guy at Yale, you could actually numerically determine that in an instance of what the per period utilities would be. The only thing I can say on a general level would be the lifetime level of utility -- at least the expected lifetime level of utility would be higher for the person who went ahead and smoked compared to the -- what his perception would be of the no smoking case.

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And if I were to argue -- I would think that because of the withdrawal, which is a crucial notion, which is sort of you think might be missing from the classical music and less plausible to have withdrawal. You could. I could imagine somebody who is listening to Bach and who is now put in a prison cannot listen to Bach and he has withdrawal symptoms. It's a little more farfetched I agree.

Whether or not the actual utility level is below where he started from, well, it's possible. That's certainly a part of the notion of the theory of addiction. That people can go into cycles where they get real highs to make up for the terrible periods of withdrawal. I mean, a heroin addict would be a more extreme version of your example where a person could literally spend a period crawling the wall or months crawling the wall because of the withdrawal, the -- it's a chemical stimulant. It's the -- as you know, it's the stock of utility over the lifetime. It's not just the flow at that point, but that's where I would say if anything cigarettes are probably more like heroin than they are like classical music.

Q. Let me go back to the seven dimensions

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of --

A. Yes.

Q. -- perception of harm of smoking. Let me try it generically as dealing all seven at once, and I will be happy to go one by one, but in case there's a general answer, I will try it that way. Can you for -- I will phrase it this way.

Can you for any of the these seven possible mechanisms through which a change in the mix of information might reduce smoking, for any of the seven, do you have a view regarding whether, in fact, a change of the mix of information in 1975 would have had such an effect -- putting aside for a second the magnitude of the effect. A change in the mix of information in terms of taking away all the statements by the tobacco companies regarding the health consequences of smoking?

A. You have got to be careful on that, and the reason why I have to be careful is that if I were to say, yes, the people accepted the tobacco company as the source of information, okay, and then used that, then I would have to logically proceed to go back to each of my seven answers previously and say if they use that information and

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they act on it, then they would have reduced their smoking, no doubt about it.

The real question is whether or not they would take that as a source of information, a reliable source of information; and so I am answering it negatively, but I am saying if the tobacco companies had withdrawn it. That's the sense in which I think you asked it, right?

Q. Uh-huh.

A. That it would sort of implicitly assume that they were listening to the tobacco companies in the first place and removing it would have been a substantial change in their information about the health risks of smoking. So there's an implicit theory. You are adding to the rational choice model which is saying a theory of information processing and prioritization where I would weigh somehow the tobacco companies' theory -- information with the government's information, and it's not obvious. You know, there are many ways that people weigh information and many theories about that which is sort of above and beyond the rational addiction model.

So I am not trying to be coy with you,

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but I think this is getting to the essence of the problem here. I will try to get back to the essence of the response, which is that I could well imagine models and -- consumers not just the theoretical model where they just discard the information. So whatever the -- whatever -- so everything you said above one through seven is perfectly correct, but it's just that it's irrelevant in this particular case because they don't view it as a source of information so.

Q. I think just to clarify, I understand and I agree --

A. Okay.

Q. -- that if nobody paid any attention to the what the tobacco companies said, then you would get no chance --

A. Correct.

Q. -- for any of those seven --

A. That's it.

Q. And my question to you is really to ascertain whether there is anybody who would be affected by the absence -- by the presence of the tobacco company statements for each of the seven? I can do them one at a time or you can deal with

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them collectively if you find that easier.

A. Well, I mean, as a hypothetical, I can say that -- you know, well, if you want to go through the list, that's fine. But as a hypothetical, I would say that to the extent that the tobacco company information was used in the decision making of the agents, that the companies were listened to or even overrode. See, there is a little bit of ambiguity about what we mean by information here and how agents would process information.

For example -- just to give you an example here. If I came along and had a problem with my health and I had a Jehovah's Witness who came to my door and told me that taking blood was bad. That's information. I get that. I process it. Actually, if I became an Jehovah's Witness, I couldn't -- I couldn't become among the I guess it's 144,000 or whatever it is if I took blood transfusions, and then later on just a week later I get knocked over, and somebody -- and I say to myself, well, here is really important information. I mean, I would argue that the Jehovah's Witness information would probably be put down very low on

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my priority and actually would not affect my behavior in the sense that since I actually might for other reasons not want be a member of the 144,000. I think they knew what they were talking about.

So there really is a crucial question here that's implicit in your question which is: What do we know about information processing by agents? That's the essence of it. What the source of authority is and what -- so the answer is in each one of the seven that if they literally took the tobacco company seriously in some way, weighted the information in so the -- and I am not quite sure what you mean by that, but I can imagine like a Bayesian way where I would say, oh, I am going to weight this information; I am going to weight that and come up with a prior on what I think the costs are --or the probabilities are of various event.

So having the tobacco companies out there is a big problem because that would give me, you know, a lot of weight, a lot of information. On the other hand, if I am sort of somebody saying, well, I discount these people. The information is out there like the Jehovah's Witness information

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about blood. Hey, it's nice to know people think that and some people think that, you know, this is true and the moon is made out of green cheese, but I don't consider it. So the real question is the information aggregation. How I would use the information that was out there in the world.

Q. Different people use that in different ways?

A. Well, there you are getting close to just a basic hole in what we know in general about whether or not people -- how people process information, but that's the essence of what you are saying here or questioning about. I think more than the rational addiction model because each version of the seven that you suggest all sort of move in the same direction. I give you information about an act. Then I -- you know, and if I really put it into my choice set, put it in and it changes my probability, then, sure, it would change my behavior in each one of those seven.

Q. Let's just take the first three more concrete --

A. Okay.

Q. -- which was the percentage of people

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who believe that smoking is likely to have substantial adverse health consequences.

A. Yes.

Q. Do you have any basis for concluding that of the people who said that they didn't believe that smoking would have substantial adverse health consequences, that none of them, none of them were influenced by the tobacco company statements in arriving at that conclusion?

A. Well, I would turn the question back if you -- again, just to clarify which would be what do you mean exactly influenced.

Q. I will take that as a question --

A. They would be aware of it. Yeah, they would be aware of it, but what do you mean by influenced?

Q. By influenced, I mean that but for the statements by the tobacco companies, they would have answered the question differently and said, yes, I do believe smoking would have had adverse health consequences.

My question again is: With that definition, do you have an expert opinion regarding whether there is any -- can you say as an expert

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opinion that there was nobody who would have answered that question differently if you had taken away that information?

A. The world is too diverse. I am sure there is somebody. Somebody will do almost anything.

Q. Do you have an expert opinion regarding whether of those people who said that they didn't believe it had adverse consequences -- smoking had adverse consequences, 5 percent of them would have answered differently had they not been exposed to the statements of the tobacco companies?

A. I don't know if it's 5 percent. I don't know if it's zero percent. I don't know if anybody knows. I mean, that's the question, that's really the ultimate question here in this litigation.

Q. I am sorry. I didn't mean to --

A. No, no, no. I accept -- I accept it. I mean, I am just saying that that's the $100,000 or I guess $100 million question, whatever, that we are talking about and that's what I don't think we know.

Q. Okay. And I will just take a shot at it. Would your answer be the same for conditions

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two through seven?

A. More or less the same, yes. That -- again to the extent that people -- I think there's a -- first of all, there's a sense in which people might seek some justification for decisions they have already taken, but I think one would have to look very deeply in the structure of decision making to say that source of information of this type would have influenced their behavior in a substantial way, and I don't know -- I haven't seen any information that tells me that's the case. So that's the ambiguity about this.

Q. Have you seen information that tells you it's not the case?

MR. WILLIAMS: Now, we have getting far -- not the case on one through seven?

MR. ANGLAND: Could you read back his prior answer. I just want to make sure I am clear on this. Actually, the last paragraph of the final answer is sufficient.

(WHEREUPON, the record was read by the reporter.)

MR. ANGLAND: And to answer your question, counsel, yes, I am asking about in one through

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seven.

BY MR. ANGLAND:

Q. Have you seen information that in your view confirms that for any of the items one through seven that removal of the tobacco companies' statements would not have had a material effect?

A. I haven't seen any evidence one way or the other.

Q. Okay. That's all I wanted to find out.

A. I mean, I can borrow evidence from other studies about other aspects, but on that very narrowly specified question, no.

Q. On that -- sort of picking up on your last comment, Dr. Harris attempts to compare an actual world with a counterfactual world, correct?

A. Yes.

Q. And the counterfactual world is a world that includes the elimination of statements by -- of certain statements by tobacco companies, correct?

A. Yes, yes.

Q. In trying to describe what that counterfactual world would have been like; that is, what smoking prevalence would have been in that

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counterfactual world, he doesn't actually look to any natural experiment where there were no statements by tobacco companies, does he?

A. Correct, no. There's no -- in that sense it's very conjectural what he is doing.

Q. He uses -- this will be my word, not his -- a proxy to an extent. One of the things he does is use as a proxy looking at situations not where there were no statements by the tobacco companies, but where there were even more statements on the Surgeon General's side of the ledger through some form of intervention?

A. Right, and that's what I was attempting to try to do is say what do we know when you give more information, which is kind of the other side.

Q. Okay.

A. But you have to be careful. I mean, just for the record, you have to be careful because you may be comparing apples and oranges in that statement. I mean, you really are doing something qualitatively different.

Q. That's really what my questions goes to.

A. Right.

Q. Putting aside your differing views

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regarding the efficacy of intervention --

A. Yes.

Q. -- I want to go just to the whole notion of using intervention studies which do not exactly mirror the counterfactual world.

A. Correct.

Q. Is that, first of all, a legitimate approach in your view?

A. It's a very controversial approach. It's a subject that I write on a lot and it's hotly debated, and I think it's an ancient problem. In fact, in my address that I gave in Sweden last -- or in December now, I was talking about that very problem. It's a problem that's deep in the center of policy and intervention evaluation and economics and a bunch of other literatures, and it's really a question of saying, well, I want a world out there that I haven't seen before or I haven't really experienced and I want to borrow from a world that I have experienced and make inferences about that world I haven't. And that's what Dr. Harris is doing, and it has -- it's fraught with danger. And there are rules, if you will, and people generally write down very precise models and say

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under what conditions does this model apply to maybe do this.

I can give you an example. I can say I am interested in say how does tuition -- suppose that I say I want to have a scholarship program, a scholarship program -- a scholarship program to help people go to college. I have never had it before in my life. So what I do is I might look for a variation in tuition. Say, well, if tuition goes up, people go to school less. So you kind of think, well, I can do it in reverse. If tuition goes down; namely, I give them a fellowship, then they are going to go to school more.

So I can look at the numbers from tuition to tell me something about scholarship. That seems defensible because scholarship and tuition are the same thing, and that may not even be true because, you know, scholarship may have prestige and a bunch of other things, but, you know, we are reasonable about these things.

Here we are asking educational -- I mean, sorry, not educational -- we are asking about informational interventions of a different sort than the notion of a company coming out and trying

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to protect itself in a public setting which is different. So information and disinformation is like taking a word and putting a minus sign in front of it, but I am not sure it's the same thing, and that's where it's more problematic to go from one to the other.

But I am doing some of that myself. I mean, that's the whole question that both Harris and I are engaged in I think in this exchange on the question of how much the information -- additional information makes. But just as a caveat, we really do want to have a recognition that the source may matter. That it's not -- it's not quite like the tuition-scholarship example that I just gave you. It's maybe like this kind of information and that that's clouded with all the associations: Big tobacco versus doctors wearing robes and having degrees of recognition in the larger society.

Q. Okay.

A. One trying to sell you a good and the other trying to save your life sort of thing. So in that sense, I think you have got to be careful.

Q. Let's assume for a second very

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counterfactually that you and Dr. Harris agreed on the interpretation and meaning of all the intervention studies. Okay?

A. Okay.

Q. All right.

A. You can say that.

Q. What I am -- given that and given that as you point out this is really a proxy for what you are really trying to measure in the counterfactual world --

A. Right.

Q. -- do you have a view regarding whether the proxy would overstate or understate the true counterfactual world?

A. I think if we were to look at information interventions -- see, there are two aspects of this. You mentioned stock flow earlier on, and I have to talk always about the intervention relative to what else is out there. So I am having a little bit of trouble in your example hypothetical by saying -- it's like having two Surgeon General's reports, and I am wondering what the value of the second one would be. So that --

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Q. That's exactly what my question is designed to ascertain.

A. Right. Right. So I mean, I guess what -- my own conclusion as you know from the report anyway, is that the second Surgeon General's report would probably have a lot less of an effect than the first one, and that to use the information about from the first Surgeon General's report to tell you what the second Surgeon General's -- especially were a carbon copy. For the -- but a second Surgeon General's report that added a little more information. See, if it were just a duplicate, then it would be like a green copy of the Surgeon General versus a blue copy, and it would be no effect, you see.

So that's where I think you have to really be careful because, you know, given what's already out there, adding the stock or subtracting it is probably not going to have any effect in the green -- you know, unless people just somehow had an adversion to green books or something.

Q. Now, in Dr. Harris' model that he uses to construct the counterfactual world, he begins with a series of difference equations; is that

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right?

A. Correct. You mean from the prevalence data?

Q. Right.

A. Correct.

Q. And then he does some mathematical algebrain combination of those equations?

A. Correct.

Q. And those equations wind up including several variables such as the quit rate, the initiation rate, the relative risks for smokers and nonsymptomatic quitters, the rate at which cigarettes would grow less harmful over time, correct?

A. Yes, correct.

Q. I understand --

A. Now, some of those are derived, just for the record. The quit rates and initiation rates are derived from some underlying prevalence rates about current and former smokers, and then he sort of takes those rates and switches them back and fiddles with those derived, but the -- you described the algebra correctly --

Q. Okay.

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A. -- it's just the order is a little different.

Q. Okay. And I understand from your report that you disagree with his analysis of what the counterfactual quit rate would have been; is that correct?

A. Yes, that's correct.

Q. And you disagree with his conclusions regarding what the counterfactual initiation rate would have been?

A. Correct, yes.

Q. Do you disagree with anything in the mathematical structure of the model though in terms of the difference equations?

A. Well, under the assumptions he makes, I don't. I have -- I mean, I didn't put this into the report, but if you are asking me now -- maybe I should not spoke to it or speak to it?

MR. WILLIAMS: No, you can speak to it.

BY THE WITNESS:

A. Okay. If I ask -- if I ask myself the question, there are a lot of very strong simplifying assumptions. You know, stationarity assumptions about rates and, you know, the world is

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a little more complicated than current and formers because we could have current -- formers going back to current. You know, people who quit and then had a problem and had to -- and essentially returned to smoking.

You know, the model is very aggregated. It could be disaggregated at various levels, but to the extent -- to the extent that it is just accounting identifies, I can't quarrel with it, no. I mean, no. So I would accept that except I would have -- you know, I would have done it a little differently maybe.

BY MR. ANGLAND:

Q. Okay.

A. Used different mortality -- it's a very, very aggregative model. The mortality rates are very aggregated, for example, and, you know, everything I know from demography suggests these things are very different across groups, for example, and I would have written down a dynamic -- I would have written down a systems of differential equations which would have had people moving back and forth between smoking and not smoking and then exiting out through death and other things. So it

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would have been a little more elaborate model, but the structure I think is -- the basic structure I would agree with.

Q. Okay. On page 6 of your report in paragraph 15, sort of in the middle of that paragraph, there's a sentence that begins with the word "for." It's right at the end of the line "for example"?

A. Teenagers?

Q. Yes.

A. Yes.

Q. In that sentence you indicate that among other things teenagers and young males are more like to discount the future at higher rates, and as a result, current money price is a larger component of their full cost of smoking?

A. Yes.

Q. Okay. Does it follow from that that compared to others, young males and teenagers are less influenced by the adverse health consequences of smoking?

A. To the extent that they are discounting the future at higher rates, the real implication of this is that the money -- current money price is

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going to have a bigger effect on them which it seems to have in the data. To the extent they are discounting the future, then remember it's their preferences; but with their preferences, they would tend to think -- to discount the future risks associated with smoking, that's correct.

Q. More than an adult would?

A. Yes, more than an adult would, that's correct. I mean, that's consistent with this sentence here, and there's some evidence I might add on this question of discounting that's been elicited by consumer surveys and like at the University of Michigan and other studies as well.

Q. Now, in paragraph 16 I just want to clarify one point.

A. 16?

Q. 16.

A. Yes.

Q. You talk about the probability of quitting having been found to increase with the length of time an individual has smoked. Do you see that?

A. That's correct, yes.

Q. I just wanted to confirm that you are

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talking about the probability rather than the probability of having quit?

A. It's actually quitting.

Q. Yeah.

A. Literally this is a hazard rate --

Q. Okay. I --

A. The established article is literally the exit rate from the state of smoking.

Q. That's exactly what you say, but in my experience sometimes people will --

A. No, no, no, but --

Q. -- waffle?

A. -- that would be different because it would be integrating it up, but actually the Douglas article that we cite here in 1998 is actually the rate, the per month rate or the per year rate.

Q. Now, in that sentence you continue and say that this higher quitting is consistent with the rational smokers tendency to give greater weight to potential health costs as they become more imminent.

Do you see that?

A. Correct.

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Q. Could that increased quitting, however, be attributable to another factor as well in particular to the extent that information about adverse consequences of smoking is a stock that increases over time?

A. You mean their private information?

Q. Yes. That individual's decision -- that that individual might be more likely to quit for reasons apart from the greater imminence of the adverse consequences?

A. No, there's a version of that model that you just announced that suggested that if the information flow is coming in just at the right rate, it could mimic this phenomenon about health risk effect. I would be questioning that only because in principle I could distinguish those two models, and I could distinguish them by saying that the -- that the model of information flow you could imagine coming at a much earlier age, and the model of health risk is typically in the late 40s -- mid-40s.

So you would have to kind of a gap in information flow that was sitting there. So I would claim information flow is probably happening

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more rapidly. So you can in principle distinguish the models, but there's a version of the models that would make that indistinguishable, I agree, and I don't find it plausible but it's possible.

Q. What is the probability that an adult male will wind up dying from lung cancer?

MR. GARNICK: Objection, form.

MR. WILLIAMS: Assuming -- well, form objection. You are assuming smoking or not smoking?

MR. ANGLAND: Just in the American population as a whole.

BY THE WITNESS:

A. I could look that up. I -- I --

BY MR. ANGLAND:

Q. As you sit here today.

A. If I had the Surgeon General's report. I mean, the probability that everybody is going to die of something is one, and now I have to go back and say at various ages I would look at cardiovascular conditions and so forth. I mean, I have to break it down cancer versus heart disease. I should know this, but I don't. I don't know it off the top of my head.

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Q. Okay. What -- and if I were to refine the question to ask the same question about smokers; what's the probability that a smoker, an adult male smoker will some day die of lung cancer?

A. It's higher for sure than a nonsmoker.

Q. But can you --

A. Not off the top of my head. I could look it up easily. I am very bad at storing facts like that in my brain.

Q. Could you approximate it as you sit here?

MR. GARNICK: I am going to object to the form

BY THE WITNESS:

A. I could, but I wouldn't put a lot of stock in --

BY MR. ENGLAND:

Q. Just to be clear, I am not asking to you take a wild guess, but sometimes when I say I don't know what Mickey Mantle batted in 1957. I knew it was somewhere between 370 -- 360 and 380, but I couldn't tell -- can't recall exactly what it was.

A. Was it that high?

Q. Yeah, Williams batted 388 that year, and

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he didn't win the batting crown.

A. The risk -- well, now you are talking about ever dying or are you are talking about dying at a point in time?

Q. Ever dying from lung cancer.

A. From lung cancer just in the general population.

Q. And now it's limited to smokers.

A. Yeah.

MR. GARNICK: Objection to form.

BY THE WITNESS:

A. I really -- I'm pressed. I mean, I hate to give a guess. I just don't know it off the top of my head. I know -- if you gave me ten minutes in my library at home, I could get it --

BY MR. ANGLAND:

Q. Okay.

A. -- or even five, but I don' have --

Q. And if I were to change the question from lung cancer to another form of cancer, would your answer be the same?

A. A very specific subform of cancer. I would have an idea of the relative rankings, for example, prostate versus lung cancer and so forth.

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Q. Right, here I am going not for relative rankings but for --

A. Absolutely probabilities.

Q. -- probability of death from that source.

A. Right. Just -- I mean, off the top of my head, I don't know the answer.

Q. How about the probability that an adult male who currently smokes will one day die of kidney failure?

MR. GARNICK: Objection to form.

BY THE WITNESS:

A. Well, I know that there's an associated risk. I mean, there are a lot of correlated risks, but again I would have to look it up from the medical statistics. I don't know.

BY MR. ANGLAND:

Q. And again, I am not asking you to make a wild guess, but you can't as you sit here today give me a, you know, a reasonable judgment or approximately what that number is?

A. Well, if I sat here and we went through all the risks, I probably could tell you, well, you know, if they are going to die of something, they

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were probably going to die, you know, maybe from some cancer. I mean, certainly accidents would be 10 -- you know, 5, 10 percent, but it's very rough I am talking about. It's worse than your Mickey Mantle range.

And if I were to say cancer versus heart disease, I think we would find heart disease to be a very major factor; but again if I were to say 40, 45 percent ever die of some disease related to heart including blood pressure and all the range of conditions of stoke versus some form of cancer, I would probably weight them like 45, 40. But it's very roughly, and the subcancers I don't know. I just don't know.

Q. Given that as you sit here today you don't -- not having those resources available to you -- know what the probability of death from lung cancer is for a smoker, can you make a rational judgment regarding whether to smoke?

A. Well, yes, I can in the sense that relative to my information set, what I can do -- see you are equating rationality with a certain model of decision making which has me sort of taking each of the probabilities and multiplying

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up. What I would say is without doing that, going back to Friedman's pool table or I guess Machlup's pool table, I would say, look, I know from lot of experience at reading around me that smoking is going to raise my risk of a number of diseases, associated diseases. How far exactly, I don't know, but remember I am making a binary decision.

So I don't really need to know the exact probability to make a decision to do it or not to do it, and that's the essence here. So in other words, if I am going to say, oh, I am going to be smoking or not smoking, then as long as I know the following fact to be the case that the probability of all diseases is higher smoking than the probability of dying of various deceases say by age 60 or some -- in other words, from an econometric standpoint, something called stochastic dominance.

If I thought that sort of life expectancies for a whole variety of conditions would be higher under the no-smoking world than the smoking world, I wouldn't need to know the exact probabilities to make that decision and be rational about it. All I need to know is one is bigger than the other. So one stochastically dominates the

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other. So I would say this healthy life distribution would be dominated by the nonsmoker. So I could be fully rational in the sense of making the right decision and not knowing the actual numbers to go with it.

Q. Sticking with lung cancer for a second, consider two people who are otherwise identical; one smokes and has smoked for a substantial period of time; the other person has never smoked and never will. Approximately how much higher is the smoker's risk of dying of lung cancer than the nonsmoker's?

MR. GARNICK: Objection to form.

BY THE WITNESS:

A. Oh, substantial. I think the ratio is like ten to one or something like that as I recall, the relative odds ratio, and I know, for example, again this is from Falise if you did smoking and asbestos, it would be like 90 to 1. It was later scaled down. There was like I believe a ten to one. If you were to look at males, this is male data. The female figures are a little different partly because women have been smoking more recently and the mortality statistics are a little

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different for them because the record is in a steady state.

BY MR. ANGLAND:

Q. Now, you have reviewed some studies by Profession Viscusi regarding perception of risks associated with smoking?

A. Yes, I have.

Q. Have you drawn any conclusions regarding whether those studies are reliable? And if you have to distinguish between different of his studies, you know, that's fine.

A. Well, for example, if you look at my testimony here, I am referring to this review of the economics and statistics paper, and then I am also looking in particular at this Journal of Political Economy paper 1990, and I am aware of those studies. I am also aware of some of the criticism which I cite by Slovic and others about asking whether or not the -- even if people know the true risks, whether or not they personalize them which is not the question you are asking, I guess, but I address in footnote 17. My understanding of this literature is that it's -- that -- I mean, I can just tell you what Viscusi

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says, but are you asking for that or are you asking for criticism?

Q. No, I know what the articles say. I am asking for your evaluation of the articles.

A. I think it's a good -- I think to me it's an interesting statement that people are overstating the risks of contracting disease. Okay. So they seem to be -- of a smoker contracting a disease. They seem to be overestimating the smoking risks. That seems to be -- that seems to be interesting. Not decisive because I am sure these probabilities are very rough that these people are answering, but it is so far removed from the probability that people are giving that it's relevant. It's suggestive that people are either at the right probability level or greater.

But the really important thing from Viscusi is, of course, to understand what the probability is of smoking versus the probability of sort of not smoking. In other words, knowing that smokers are more likely -- and I don't think Viscusi actually -- so I think Viscusi is very interesting in the sense of providing you with

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information about the fact that people seem to be overestimating the probabilities, and it does suggest -- at least suggestive that people are somewhat informed about these. His studies aren't complete though because he doesn't estimate the other half of it.

Q. The other half being?

A. Probability of nonsmokers.

Q. If -- do you have any view regarding -- strike that.

Presumably if people were surveyed and asked not just about the risk of mortality due to lung cancer for smokers, but the risk of mortality for all sources identified separately, it should in theory add up to 100 percent; is that right?

A. Well, you have got to be careful. I mean, I have worked on these contingent claims -- not worked on them but I may have participated in these elicitation studies, for example, for Exxon and the oil spill. I went down to work for the government ten years ago, and I saw these people adding up sums of money that it was very much framed depending on what you framed the question by. So the numbers didn't always add up.

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It seemed to me if I recall in that study people were willing to say that all environments -- all damage to the environment should be taxed at a rate of, you know, like 50, $100 billion. It didn't matter how many risks you added up. So it's like -- it was somewhat contingent on the framing of the question.

Q. Well, if the question were framed to be what is the probability that a smoker will die of each of the following 500 conditions --

A. Yes, and mutually exclusive and you gave all of those, I think most people if they were forced would ask to have the probability sum up to one. You would think. You would hope, but I don't know if there's a guarantee. If you ask them like one or two of them, there's no guarantee, and especially if you are asking one or two at one time and one or two at another time and then they could add up to 300 percent for sure.

Q. Let's go back to the mutually exclusive qualification that you gave there.

A. Yes.

Q. Let's assume that some of the conditions are not mutually exclusive in that a person's death

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could be the result of the convergence of two factors.

A. Okay. But I would call that a third factor in some sense. I would say -- so I would say A, B and A and B as a factor, but yes. That's how I would make it mutually exclusive.

Q. Okay. Let's assume that the way the question is asked it doesn't find their --

A. Fine.

Q. -- converged effect as being a separate factor. In that case should the totals add up to 100 percent?

A. Well, see the question is how they are answering. See, so if I told it's a probability of A and if by A -- this is like a Venn diagram you are asking, well, if I have A and then I have B and then I have the overlap and you are counting the overlap twice, it could be greater than 100 percent because what you were doing was getting A and B through the A side, A and B through the B side and not sort of counting the overlap.

That's why I wanted to make it mutually exhaustive sets. So it could be A alone slash B, B alone slash A and then A and B together, and that

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would be -- those should add up from the sense that you forced them by logical consistency.

But no, if I said what's the probability of dying of cancer and then what's the probability of the dying of some other disease say stroke, I could have easily had an interaction between stroke and cancer. The marginal answers could add up to more than 100 percent, right? That would be possible because I wasn't being careful in the overlap set.

Q. In your discussion -- strike that.

A. Sorry.

Q. In your discussion of intervention studies --

A. Yes.

Q. -- you refer to among other things cross-sectional studies?

A. Yes.

Q. Okay. Now, in those -- and your conclusion is -- and I am generalizing here. Is that in general the intervention studies didn't show much of an effect, if any, of intervention to try to induce people to stop smoking. Is that fair?

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A. When I look at the intervention studies and have the long-term follow-up across a number of studies, the short-term effects and for certain small groups and certain subgroups there have been some weak effects, but the body of the evidence, the thrust of the evidence is that interventions done in the '80s and the '90s where a lot of the work is done, even the stuff in the mid-70s has shown fairly weak effects about the interventions which is a major problem for social intervention research on this topic.

Q. Now, let's focus on the subset of those studies that are -- involve cross-sectional analysis.

A. Do you mean those that have -- well, I am not quite sure what you mean by --

Q. Okay. I am trying to exclude the cohort --

A. Okay, yes.

Q. -- economies for the moment --

A. One COMMIT report versus the other.

Q. Excuse me.

A. Well, there's a COMMIT cross-sectional and there's --

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Q. Okay.

A. Yeah, but I guess the question is do you want to make the distinction cross-sectional or do you want to make it experimental versus nonexperimental?

Q. Yeah, let's take it observational versus experimental.

A. Okay. Fine.

Q. And let's focus upon the observational.

A. Okay. The observational cross-section studies. Okay. Fine.

Q. Now, if the individuals we are studying in those studies differed in ways that were not accounted for in the study, that could lead to erroneous results of the study, correct?

A. Yeah, either way. That's the problem we were talking earlier when we were discussing the Rubin question, and that's one of the fundamental problems of causal inference from cross -- non-experimental data.

Q. Okay. And do you consider these studies that you cite in this report, these cross-sectional observational studies, do you consider them reliable?

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A. Well, I consider them in the sense -- reliable in a sense that they represent the accumulated knowledge in that field, but I don't consider them as reliable as, for example, the randomized trials like COMMIT and the other studies where either communities or individuals were assigned at random to different studies. They are not as reliable. They are inherently controversial.

To the extent that people using different, non-experimental methods tend to find the same results, which are like no results, then I tend to start believing them, but I believe them more because I compare their results from the results from the experimental studies, that's correct.

Q. Focusing upon the observational studies, some of them report R squareds; is that correct?

A. Some of them do, yes.

Q. Okay. And some of those R squareds are down at 10 percent or below; isn't that right?

A. That's true, yes. It's very common with individual data.

Q. What does that suggest about the

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reliability of the study?

A. Nothing. The reason being that we can imagine there's fundamental measurement errors say in the dependent variable, and I can still isolate a true effect. So even if I had an R squared of .01, I could still imagine estimating a strong causal effect. What it would really tell me is that if I wanted to predict this variable, you know, that I am only getting a piece of the story. There are a lot of other factors there, but it doesn't cast doubt -- intrinsically cast doubt on the structure of the causal relationship that's found.

Q. And the same could be true if the dependent variable were expenses, medical expenses and you are were trying to analyze the effect of smoking on medical expenses; is that right?

A. Well, you are saying if the R squared is low?

Again, see, there you get to a different problem. If I wanted to say what's the effect, same remark would apply dependent variable being anything. If I wanted to go off and develop a prediction equation like in a damage settlement or

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something, then I would start being concerned about the R squared for the model; but when I am down at the level of saying is there an effect, then I don't -- see those are two different issues, right.

I am just asking does this thing rise above the noise level in the data. That's fine. I can say there's a big effect in a statistically significant sense. Does it explain all the variation? No. And now if I really want to go into a prediction exercise as opposed to an inference exercise, then I am in big trouble I think when I have an R squared of .01 because there's so many factors that I have left off that I then get concerned about making forecast stream, that's right.

Q. If your model were a model where the dependent variables being estimated -- being regressed against a number of -- strike that.

You are familiar with the fact that there was a smoking intervention program run by the State of California for a number of years?

A. Oh, yes, I looked at that study.

Q. And that smoking intervention program involved at least two elements, one of which was a

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tax, effectively a price change?

A. Correct.

Q. And another of which was trying to provide information --

A. Correct.

Q. -- to people?

A. Yes.

Q. Okay. Do you have any view regarding whether the provision of information as part of that program had any effect upon smoking prevalence in California?

A. Well, there's studies that address that question.

Q. I have read some of them. I am right now asking for your opinion.

A. Well, the one -- okay. My opinion is probably based on the study of Keeler, et al., and it's partly because I -- Keeler visited Chicago and visited Chicago many times, and I have a very high regard for him. So I tend to weight his evidence more, and he is more careful. He has an econometric study that indicates that the -- in the early stages of the California intervention, there was a substantial effect; and he sort of

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weights it more -- substantial effect being -- in reduction of smoking, and he tries to allocate it between intervention information effects and intervention price effects. And I believe it's more like two-thirds to three-fourths of the whole effect is like price effect in his study, and he finds some evidence of information effects reduction.

The thing that's kind of worried me a little bit about that study is the fact that, you know, post-90s that we see a rise in smoking among some of the very -- you know, especially the teenagers and the youth in the state. So the evidence is not entirely happy evidence for information. Information is out there. There are price effects. We are still seeing in the case of the substantial intervention an increase in smoking which is very troublesome to many people in the public health community, but I think that's a good study.

Q. Okay.

A. An --

Q. I am sorry.

A. No.

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Q. Are you familiar with a program Massachusetts undertook to discourage smoking?

A. I am aware of that. I have discussed that.

Q. Have you formed any opinion regarding whether the provision of information that's part of the Massachusetts program had any impact upon smoking prevalence?

A. I think the evidence there is relatively weak from my reading of these studies, that it's relatively weak, and when I put the Massachusetts study in context with all the other studies, I tend to say there isn't such evidence. There are price effects there in Massachusetts as well. And one of the biggest problems which is what you were referring to I believe a few hours ago in your testimony -- in the deposition hearing was -- was the question that we have dynamics big time in these studies; and the analysis of the Massachusetts data unlike the California data I don't think has quite captured the dynamics yet.

I have been a little more suspicious about the Massachusetts studies only because I don't think they were analyzed as carefully as the

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California studies. I mean, there are the several California analyses that I think are more comprehensive, but the Keeler study I think has yet to be done, a Keeler-like study has to the done yet for Massachusetts.

Q. Have you looked at the article published by Dr. Harris and others in the year 2000, late in the year 2000 --

A. Yes.

Q. -- regarding the Massachusetts --

A. I did and it was based on the same -- those were my reservations about that was the question of treatment of dynamics and --

Q. Okay. Could you articulate more specifically --

A. Do you have the article here with you? I would have to look at it to be precise.

Q. I don't and --

A. So I am not -- so I would hate to sort of go off on a tangent to sort of cooking things up, but there was a general point that I do remember, and that is precisely because I was so sensitive to what was going on and saw the effect of careful controls for dynamics way back in

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the -- you know, in the stuff on the fairness doctrine.

Then more recently in the context of the California study that I was a little bit surprised in the study. Maybe it was just because it was so terse. Maybe the format was such they couldn't really amplify, but they didn't really discuss this very distinction you were making: Stock flows, the intervention, the timing, the depreciations, a whole set of serial correlation problems that I think were discussed in California with time series methods that weren't -- so that led me to be a little skeptical just because the raw analysis of California without taking into account the dynamics seem to suggest much bigger information effects. And when I did it -- when people did it -- did reanalyze the data allowing for dynamics, they found much less evidence of intervention effects, information effects.

So that's what leads me to be suspicious of it. I am not saying it's wrong, but I would have to look at it to be more precise because it's been a little while since I have looked at that study, a few months, but it had that problem. So

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that general point I would make from memory.

Q. And is there any other, you know, criticism or question about the study that as you sit here today you can recall, the Harris study?

A. No. It's just -- what I am saying is the dynamic treatment -- and so there's several aspects to it. There's the sense of the cumulation of the knowledge, the intervention itself is an ongoing thing. It takes -- you know, there are break-in effects and then there are wear-down effects. So, you know, that's not modeled at all. There's a serial correlation in the observations, and then the dynamics themselves, the model of addiction, a model of smoking behavior, the very model of addiction has sort of current, future and past prices in it. And what I recall there wasn't any such distinction made in that study.

It was very much like a point in time intervention study like you were suggesting correctly would be a bad thing to do for the -- for the disinformation studies. It's kind of like an event history study without a dynamic stock and timing structure to it.

Q. Let's go back to the -- or not back.

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Let's go to the cohort studies -- cohort-type studies that were used for an analysis of the effectiveness of intervention that you cited in your --

A. Which one? You mean COMMIT or --

Q. COMMIT was one. Well, let's just leave it at COMMIT. It will simplify things.

A. Okay. Fine. That was a more -- that was a study -- in the COMMIT study that's the case -- that's the analysis for the heavy smokers. That's the one they really emphasized in their final report. So when people talk about COMMIT, it's usually that longitudinal study where they randomize now across sites in that study too. So it's not just observational. It's observational within a community, but it's randomized across communities, right, in the COMMIT study if I recall.

Q. Now, it has been a while since I read that one, can you remind me how the -- you know, what the control versus -- how the cohort analysis was done. What was compared to what?

A. Oh, what they do is they find -- this is now as I recall correctly community-level

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interventions. And so what they do is they find paired communities chosen at random which one got the treatment and which one didn't, and then they would do the sequence of interventions which were actually very rich. They are numerous versions of the interventions. You know, informational interventions, visits by doctors and so forth, educational activities as well as the media events.

But they would then look at the communities and look at the groups of individuals who were surveyed. I think they call it the end -- I have forgotten -- the end point people they give them the name, but basically they terminate -- they follow this group of people over time, these cohorts of people and ask to what extent was their smoking cessation.

Q. And the cohorts in the -- in a given treatment community --

A. Yes.

Q. -- was chosen to be similar in certain respects to the cohort chosen in the control community; is that correct?

A. Yes, yes. Now, in a sense, see you raise an interesting point because what you have

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got is the communities are randomized at the community level, but you can still have some notion that there might be a few more people of one type than another in the subcommunity. So that's what you are getting at.

Q. That's where I am going.

A. I understand. But they try to control for that by putting in things like blue collar, by education. They try to stratify within the communities to make comparables.

Q. Okay. Did they control for enough of those variables to make the cohorts sufficiently comparable that you can rely upon the results of the studies?

MR. WILLIAMS: Object to the form.

BY THE WITNESS:

A. Well, they had a lot of data there available from common studies. Whether it's enough, I can't say because I -- I would have to know the true model to know if it was enough, but I will say it's a very extensive set of controls that they have in the reported papers that I have read: Things like, you know, age and so forth, race and sex. Certainly the main demographic categories.

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They don't have things like IQ. They don't have things that you might want to put in for sure no doubt but --

BY MR. ANGLAND:

Q. Do they have personal medical history as one of the controls?

A. I would just have to look at it. Do you have a copy here?

Q. I do with my marginalia all over it. So I am going to try to avoid that. But if need be, I can do it.

A. But as I recall, they collect such information from a subset, but I just don't remember the analysis that does or doesn't. I would have to look at it.

Q. The bottom line, do you have an opinion as to whether those cohort studies were reliable?

MR. WILLIAMS: Well, you are talking about the COMMIT?

MR. ANGLAND: The COMMIT.

BY THE WITNESS:

A. Yeah, the COMMIT study I think is -- you know, again, here I am outside observer, an informed outside observer looking at the medical

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literature. This was viewed as a revolutionary study by that literature because of the randomization across community. It was a community-wide intervention and meant that a lot of the standard problems associated with the interventions in the past were alleviated.

How? Because you basically had a saturation. At least one community was quarantined from other. There was no close proximity of the two communities. One of the biggest problems here in this area is you get some people exposed; some people not exposed; they leak; they transmit. That's been avoided. Whether or not randomization of 22 communities is enough of a sample size? No, there are questions you could raise about every single study, but I consider it to be pretty interesting -- and again when I -- now to go away from COMMIT. I put it in the larger shelf of all the studies. It seems to be finding consistent evidence from other --

BY MR. ANGLAND:

Q. Well, I am sorry. I didn't mean to cut you off.

A. Yes, go ahead.

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Q. Why is the fact that other studies came out sort of the same way relevant to appraising COMMIT?

A. Well, I think take, for example, this Hutchinson study which I guess we now have introduced. Are you familiar with that? I don't want to spring surprises on you.

Q. Is that the December 2000 --

A. That's the December 2000. You know that study. Okay. Fine.

See, now there's a very different kind of intervention, a different group of people. You know, the COMMIT study was looking at communities and targeting heavy smokers, primarily adults. This is like targeting a totally different group of people, people in school, following them over a long period of time. Again, it has a cohort-like, a longitudinal feature to it, and the finding that this kind of analysis shows weak effects also again with the same -- is suggestive. You know, you gather what you can. You take little bits and pieces from one study or another.

Partly, I always look at the literature -- you know, I respect an internal

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discipline within the literature. So when I see sort of the leading editors -- you know, editors of leading magazines saying, well, this is the state of the art, the gold standard I think was the term used for Hutchinson or something like it also for COMMIT. Then I tend to say, well, I may have any own reservations about this, but relative to the epidemiological literature forming the whole health debate, this is top of the line.

I could -- I would have my own reservations. Many people have criticized me for being too negative on everything when it comes down to studies. So I personally would assert much more, you know, individual detail and so forth and so on. But I think there's a body of conclusion that seems to emerge from these and the earlier studies, the non-experimental studies. And there's just a general principle as you put this stuff together, it cumulates. You find it here. You find it there. You find it across a whole body of areas. If it's cumulating then -- and all pointing in the same general direction, I start to believe it more.

Q. Did the COMMIT cohort study use

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propensity scores?

A. Well, remember the structure of the study was -- for the community -- for the intra -- for the inter -- in the published reports that I have read, I just remember there's a summary of a bunch of other auxiliary studies, but remember the main thing is like looking at mean differences across communities. I would have to look at the paper again. I don't recall that the main findings that I examined were based on a propensity score study. That's my knowledge, but I do remember they said there were a bunch of auxiliary studies, and they may have used it. I don't know.

Q. Let's assume that when a cohort from the treatment community is compared to the cohort in the control community with respect to some measure of smoking prevalence at the end of the period.

A. Right.

Q. That -- let's take the extreme case. Let's assume that the control community actually has shown more of a decline in smoking prevalence.

A. Yes, which happened in some of the studies.

Q. Right. In that case, it would be

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possible, would it not, that if the -- if there were not a perfect match between the people in the cohort in one community and the people in the cohort of another community and if you were to break the cohorts down into subsets by age, gender, whatever other characteristics -- you know, a dozen other characteristics, it would be possible, would it not, that, in fact, for every single such subset, the intervention community or the treatment community I guess it should be called would have a greater decline in smoking prevalence than the control community?

A. You are talking about some form of the Simpson's Paradox or something here?

Q. I am not familiar with the phrase Simpson's Paradox.

A. No, but you are asking can the components individually decline more than the overall group? I mean, it seems like a technical --

MR. WILLIAMS: That was his question.

BY THE WITNESS:

A. That's your question, and that's Simpson's Paradox and the answer is, yes, that's

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logically possible and a matter of much debate because it depends on what you weight, how the weights are affected here in the different groups because you are comparing different groups, right?

BY MR. ANGLAND:

Q. Let me just make up an example and you tell me if this is Simpson's Paradox --

A. Right. Okay.

Q. -- since I am not familiar with it.

A. Fine.

Q. Let's assume that in community -- that there are two subgroups of the population --

A. Right.

Q. We will cal them male and female for present purposes.

A. Okay. Fine, yes.

Q. And that whatever we are trying to measure is -- in community A is 50 percent for males and 5 percent for females.

A. Uh-huh.

Q. And in community B it's 40 percent for males and 4 percent for females.

A. That's Simpson's Paradox you are talking about, yes.

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Q. Okay. I didn't even get to ask the question, but it could work out that notwithstanding that community A --

A. Yes.

Q. -- is higher than community B for each of the two --

A. Correct.

Q. -- but community B winds up with a higher weighted average?

A. Correct, correct because of the weighting. However, I would say in the COMMIT study in particular they do disaggregate at least by the major -- major groups.

Q. By how many variables do they use to disaggregate?

A. Well, certainly male and female and certainly like occupational status and certainly other groups as well. That's right. But I mean, you could say does it exist? Here is where you would start running out of data I think. I mean, what you would like to do or something I would like to do as well would be to go back to the data and look at all the little subcategories, and say -- the trouble is there you would run into other

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problem.

Q. Degrees of freedom?

A. Degrees of freedom. You just have too many -- you just have too little data to essentially make observations about any group, and then you would be now at a noise level by disaggregating. To my knowledge, there wasn't a study that I read -- I can't say I have read every single report -- that disaggregated it so finely, but to the level they did disaggregate, the phenomenon you are worried about didn't show up. You would kind of think it would show up with some level, right. So but logically possible what you are saying.

Q. For my own edification, who was Simpson?

A. I don't know. To be honest with you, I know the name and it's much discussed.

Do you know Simpson?

MR. GARNICK: Homer Simpson?

THE WITNESS: No.

MR. GARNICK: That's the only Simpson I know.

THE WITNESS: This is a different Simpson.

BY THE WITNESS:

A. All I know is it's been called Simpson's

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Paradox, and I see it in statistics journals from time immemorial. It's a classic example --

BY MR. ANGLAND:

Q. Back in high school debating, we called it the fallacy of composition.

A. Yes, the fallacy of composition. Yeah, but the statisticians give names of their own actually, Simpson's Paradox, but it shows up a lot actually. It shows up a lot. In fact, it showed up in the discrimination case in Berkley very -- in fact, in exactly almost the form you gave it for males and females and admission into colleges -- subcolleges at Berkley.

MR. WILLIAMS: Joe, how are we doing on time and lunch and those sorts of things? What do you want to do?

MR. ANGLAND: Let's go off the record for a minute.

THE VIDEOGRAPHER: Going off the video record at 12:36 p.m.

(WHEREUPON, at 12:36 p.m. the deposition was recessed until 1:00 p.m. this date, 2/17/01.)

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UNITED STATES DISTRICT COURT

EASTERN DISTRICT OF NEW YORK

BLUE CROSS AND BLUE SHIELD OF NEW JERSEY, INC., et al., Plaintiffs, vs. PHILIP MORRIS, INCORPORATED, et al., Defendants.) C.A. No. 98 Civ. 3287 (JBW)

2/17/01

1:15 p.m.

The deposition of JAMES HECKMAN, Ph.D. resumed pursuant to recess at Suite 2900, 30 North LaSalle Street, Chicago, Illinois.

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MR. JOHN B. WILLIAMS,
appeared on behalf of Defendant
R.J. Reynolds;

ARNOLD & PORTER,
(555 Twelfth Street, N.W.,
Washington, D.C., 20004-1202), by:
MR. MURRAY R. GARNICK,
appeared on behalf of Defendant
Philip Morris, Inc.

ALSO PRESENT:

MR. CARY DAVIDOW, Videographer.

REPORTED BY: ANDREA L. CARTER, CSR No. 84-3722.

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THE VIDEOGRAPHER: Going back on the video record at 1:15 p.m.

JAMES HECKMAN, Ph.D.,

called as a witness herein, having been previously duly sworn and having testified, was examined and testified further as follows:

EXAMINATION (Resumed)

BY MR. ENGLAND:

Q. And, Dr. Heckman, you realize you are still under oath?

A. Yes, I do.

Q. Okay. Can statistics be used to prove that something is the cause of something else? Not to get philosophical with you.

A. That's a pretty deep question you are raising there, and the answer is some people would say no, so if you took an opinion poll. I think they can, but I think it requires a lot of care, and so the answer is yes. And, yes, provided you have a clearly articulated model and you have a clearly articulated source of variation within the model that generates the cause.

Q. For the group of people who would answer the question, no, that statistics can't be used to

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prove a causal effect --

A. Yes.

Q. -- do some or all -- would some or all of those people agree that statistics could be used as evidence that might permit one to infer whether or not there is a causal effect?

A. Well, no. I mean, there's some extreme versions. I mentioned this person Dawid who published in a major statistical journal saying, no, the idea of a causal effect was meaningless and the idea of using statistics would be meaningless, but generally speaking there's a very big demand -- appreciation, acceptance of the fact that you could use statistical evidence at least to suggest.

So many people will write down a correlation and then say correlation doesn't imply causation, but it suggests a causation. And if it's strong enough, it suggests the cause might be at work. So the majority of people I think accept the -- that statistics can be used to -- the majority of academics anyway and I think scholars across a variety of disciplines.

Q. Let me revert to a topic we talked about a little bit just to complete it.

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Do you have an opinion whether smoking tends to increase the person's expected likelihood of having an automobile accident?

A. Cause associated, yes. Cause, no. I mean, there are studies that are showing that a whole series of risky behaviors are associated with smoking, but causation, no.

Q. Okay.

A. I think that's very speculative. Most people would say a third factor was at work causing people to be riskier and taking -- automobile accidents are taking liberty with an automobile and in smoking and in using drugs and other things.

Q. Are you aware that if any mechanism through which smoking itself could actually or might actually cause an increase in automobile accidents?

A. Oh, I suppose you would concoct a mechanism. It wouldn't be very plausible or widespread, but you could have people smoking and a cigarette drop out of their mouths into their crotch or something. I mean, but realistically I don't see that as a major factor.

Q. Let me, and actually I suppose I have

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heard people say that he could drop and burn their hands.

A. Burn their hands, right.

Q. Let's put aside burns.

A. Right, yeah.

Q. Not doctor, just the burned hands and the like.

Are you aware of any mechanism through which smoking increases the likelihood of having an automobile accident? Again putting aside the possibility of burning one's self.

MR. WILLIAMS: Well, do you mean the possibility of diversion as well? You are not limiting it just to burns.

MR. ANGLAND: Okay. That's fair.

BY MR. ANGLAND:

Q. Let me -- in fact, deal with that, are you aware of any mechanism through which the medical or health effects of smoking might render one more likely to have an automobile accident?

A. Well, let me put it this way, I could imagine a less concocted mechanism by which the following happened: Somebody smoked; as a result of smoking, they were now at greater risk for heart

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failure, stroke or for that matter they were in the last stages of lung cancer and became less capable of understanding -- responding to stimuli or just being adapted to the environment and, therefore, could engage in an accident.

Q. Do you have a view regarding whether a -- the expected medical costs for a woman about to deliver a child would be increased if you were a smoker as compared to a nonsmoker?

A. Well, I think the study by Manning, et al., again, I have to look at the study, but they look at the actual cost of smoking among other costs of risky behaviors, but there's certainly general evidence, but on the specific -- I haven't seen -- I don't recall. I have seen, but I don't recall the number on -- associated with that effect. I think it's probably -- I know there's correlations with miscarriage and so forth. So that would lead one to suspect that there would be a higher rate of miscarriage and, therefore, probably a higher cost associated with a smoking mother than a nonsmoking mother.

Q. The same question with respect to gunshot wounds. Assuming that a person has a

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gunshot wound, holding everything else constant, do you have any reason to believe -- do you have an opinion regarding whether the smoker's expected expenses would be the same, greater than or smaller than the nonsmoker's?

A. Well, for the same reason, to the extent that smoking would incapacitate your health and make you less robust, then you might imagine a person who was already at the verge of death on a heart problem or stroke or stress or some kind of blood pressure say might, in fact, be more likely to die of a gunshot wound because their recovery would be impaired. So I can imagine it, yeah. I am not a doctor though. So I don't know if I have studied it so.

Q. And let me just modify that last one to hypothesize that the gunshot wound victim or victims, if we are comparing a smoker to a nonsmoker, don't wind up having an ancillary condition like a heart attack or something that turns one case to be fatal and the other not.

A. Right.

Q. But in both cases three weeks later the person is out playing basketball in the gym, and so

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that there isn't some dramatic side effect.

A. No, I understand --

MR. WILLIAMS: Are you asking him to imagine a situation?

MR. ANGLAND: I am asking --

MR. WILLIAMS: This is not part of his opinion in this case.

THE WITNESS: No, it's true. It's not.

BY MR. ANGLAND:

Q. Well, I think it has something to do with the area of rational addiction in a roundabout way.

A. Okay.

Q. It's oblique but it's there.

A. We will take it. See how it goes.

Q. All right. So the question was again assuming that you have got two gunshot victims, one a smoker, one a nonsmoker, and the both of them three or four weeks later have recovered well from their injuries and neither have had a major side effect like a heart attack or a stroke, do you have any view regarding whether the expected expenses for the smoker differ for -- differ from the expected expenses from a nonsmoker?

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A. There I am pushing any knowledge that I have. It's not something that I know much about to be honest. I would guess --

MR. WILLIAMS: Well, let's not guess, Doctor.

THE WITNESS: Yes, I agree.

BY MR. ANGLAND:

Q. Let's talk for a moment about studies of advertising --

A. Yes.

Q. -- and their impact upon the consumption of cigarettes.

A. Yes.

Q. Now, as you mentioned in your report, there are a number of studies that look for a correlation between advertising expenditures and smoking prevalence. Is that fair?

A. That's correct.

Q. Okay. Now, is it possible that the effect of advertising, if any, upon somebody's decision whether to smoke is a lagged effect?

A. Yes. In fact, the literature models it that way.

Q. Okay. Does all the literature that you cite model it that way?

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A. No, the better studies do.

Q. I think you have answered my next question which is: Should they model it that way?

A. Yes, yes.

Q. Okay. Would -- and I realize there may be a relationship between the last question and the next one, but should advertising be treated as a stock, a flow or both?

A. Oh, I mean, the advertising knowledge should be treated as a stock. There's a well-established tradition going back 40 years in economics modeling advertising, and the data analyses based on it suggest that it should be -- I mean, there's a flow that comes from information. So you can augment the stock, but the stock is a very important factor in advertising.

Q. And that would be a deficiency of a study if it didn't consider the stock aspect of advertising, right?

A. It would really limit its understanding unless you did the following: This is where I come back to a point I made earlier that I could have a study where I did not use the stock because I change. So if I reformulate the model in terms of

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levels, I mean, from levels to changes in levels, first differences like in -- then I can essentially difference out the stock and get back to a flow.

So in that sense, I could get by without putting in the stock, but then I would have to change it. The idea of level of advertising -- current advertising on level over variable would be problematic, but level -- change in the level of advertising on change in the outcome, that's not problematic, and I think that's justifiable.

Q. Is it your opinion that the level of advertising has absolutely no effect on the amount of cigarette consumption?

A. Well, how do you define advertising?

Q. Let's restrict it for the moment to what I would call -- this may not be answering your question -- brand advertising, people trying to persuade you to smoke Marlboros or Camels or --

A. No, but I guess the question is which form of advertising?

Would it be -- would you talk about -- what form of advertising do you have in mind is what I mean?

Q. That's one of the advantages the

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questioner gets.

Are you aware, sir, of any form of advertising that may have the effect of increasing smoking prevalence?

A. Well, only in the following sense that I am aware that some advertising -- something that's currently called advertising by the FTC and in the reports of the tobacco companies' file consists of price discounting. I mean, you can lower the price. I would not call that advertising in the classical use of the term.

Q. And I was not in my question.

A. Okay. That's why I asked the question.

Q. Okay. I didn't understand what you were --

MR. WILLIAMS: Why don't -- if you want to do this, why don't you talk about print advertising, something like that because the numbers you use in terms of advertising include a lot of promotional material.

BY THE WITNESS:

A. Yeah, I want to separate out promotion -- price --

BY MR. ANGLAND:

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Q. That's fine. If you had said promotion, I would have understood where you were going.

A. Yeah, that's what I am saying exactly.

Q. Okay. Let's just make it clear when I talk about advertising here, I am talking about essentially print advertising. I guess there was a time where there would have been more media advertising as well --

A. Right.

Q. -- which is another distinction I thought you might have been talking about --

A. Right, no, but the media --

Q. -- but the classic sense of advertising as opposed to promotional discounts, things that affect price.

A. Now, there are some studies --

MR. WILLIAMS: Well, let's have the question.

BY MR. ANGLAND:

Q. Yeah, the question is: Is it your opinion that -- that traditional form of print and media advertising have no effect on smoking prevalence?

A. It's my opinion based on the literature -- careful reading of the literature and

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even surveys in the literature that the advertising effects on the level of smoking prevalence, not brand choice, but the level tends to be -- is negligible, zero.

Q. Okay.

A. And that's not to say there aren't one or two studies that claim to find an effect, but I would say among the studies that I would consider to be the strong studies, the ones that are well-received in the literature, there's no effect.

Q. Let's assume for a moment, and I realize this may be counter to your conclusion, but let's assume for the moment that advertising -- that the quantity of advertising does have a positive effect on overall smoking prevalence, but that there are diminishing returns. So that as you add more and more advertising expenditures, there's a smaller and smaller increase in prevalence.

A. Okay.

Q. Accept that as a hypothetical.

If that is the case, is there a possibility that in at least some of these models they would miss that effect because of the decreasing returns to advertising?

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MR. WILLIAMS: Which models are you talking about?

MR. ANGLAND: The models that Professor Heckman --

MR. WILLIAMS: I think he has referred to studies.

MR. ANGLAND: That's fair enough.

MR. WILLIAMS: And anyways, I am going to object to the form because it presents a hypothetical directly inconsistent with his opinion.

BY MR. ANGLAND:

Q. Well, just to be clear, I am asking whether the studies you rely upon may have missed some of the effect of advertising if, in fact, the situation I described were true?

A. What you are describing in the hypothetical is the case where you think there should be non-linearity. It's called non-linearity. Now, to the extent that many of the studies put in advertising in the form of -- with a measured non-linear transformation like a logarithm or something, they already capture the possibility there would be something like -- a

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form of diminishing returns.

So to the extent that most competent analysts who look at these studies will try logs and levels, they will look -- looking at the logarithmic specifications of these equations with advertising stocks would tend to be -- would capture your effect, at least one form of diminishing returns. To the extent that the model was misspecified, that there were really steeply diminishing returns and there was a non-linearity that wasn't captured, then, yeah, it's possible.

But if you look it the books by Schmalansee, if you look at a lot of the companion literature, there's a lot of experimentation with non-linearities, functional forms. So that people try to cover themselves against that possibility. The recognition of diminishing returns is a real phenomena and people worry about it in the literature. They worry in the best studies.

Q. Is -- in terms of the ways one might model it, is it possible that the shape of the curve is a hyperbola?

A. A hyperbola between some outcome measures, say, purchases and -- yeah, I mean, in

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taking logs, you take a hyperbola, and you get like a straight line. So you can imagine that.

Q. So one possible model would be to have -- when you measure the effect of advertising on say the percentage of the population that smokes, is to have -- as advertising increases, to have the percentage of the population that smokes increase up to a point but for them to -- for it to approach an asymptote?

A. Yeah --

MR. WILLIAMS: Object to the form.

THE WITNESS: Can I answer it or?

BY MR. ANGLAND:

Q. Yes.

A. No, it's possible, it's possible. I don't recall people experimenting with that kind of -- in other areas, there are notions of thresholds or limits I should say where you get a logistic-like curve where reach a limit and you just don't go beyond it. You are taxing my knowledge. I would have to look at Schmalansee's study and the companion studies. It's not a -- it's not a functional form that's emerged as a major functional form. I have seen some

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experimentation.

Q. Actually, my question was going to be can you recall that being modeled at all?

A. Hyperbola?

Q. Yes.

A. I would have to look at the studies, but it doesn't jump out in some of the major studies.

Q. If, in fact, the asymptote was at a level below the level of spending -- the asymptote was reaching a level of spending below the level of spending in the lowest -- in the year with the lowest advertising spending, there would be no way that the model would pick up the effect of advertising on consumption even if there was such an effect?

MR. WILLIAMS: You are talking about the econometric studies?

MR. ANGLAND: Yes.

BY THE WITNESS:

A. I am not sure what you are saying. I mean, here is my point. You are saying that I am now at a level where there's no effect.

BY MR. ANGLAND:

Q. Yeah, let's --

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A. I mean, in the model I would show no effect.

Q. Correct.

A. And so if I used an econometric model and I fit it on a subperiod where there's no effect and I get the right conclusion, wouldn't I, which would be to say there's no relationship between the outcome and the advertising.

Q. Let's -- let's turn it into a -- let's take an example. I am just going to pick completely arbitrary numbers here --

A. Okay.

Q. -- for the example. Let's assume that it were the case that as the tobacco industry increases its spending and advertising from zero up to $100 million per year --

A. Okay.

Q. -- just to use a round number, that that has a significant effect on increasing the number of people who smoke, the percentage of people who smoke.

A. Okay, let's say.

Q. And let's assume that's the reality. That we know from some higher source that if we, in

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fact, got advertising down to that low a level under 100 million, you would see a lot of people not smoking who would otherwise smoke.

A. Okay.

Q. But now let's assume somebody wants to figure out whether advertising has some significant effect on smoking --

A. I see what you are saying.

Q. -- and they are given a data set, and in every year there's at least $400 million of expenditures.

A. And I agree with you, that's possible. However, I would just say this: That if we look at the time series, we see enormous variations. So I mean, even though what you are saying is a logical possibility, if we just look at the period say '75 to '83, you are getting a growth of 30, 40 percent. I think I say that in the testimony here -- the report. That essentially -- so, yes, but I could certainly agree with you that over a certain range -- I mean, that's just a general limitation of any -- any regression study, that there's a range over which all the data lie, and then we to go outside the data to get the real effect.

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Q. It's kind of like the early period of Dr. Harris' prevalence study except you don't even have the two points, right?

A. Right, yeah. It's like you literally lack the variation to identify the function, and then it's conjectural. It's exactly the same thing. I am glad you said it. It has the same kind of speculative quality.

Q. Shows I listen.

A. Yeah, it's the same idea.

Q. Okay. Now, a number of these studies as I think you said try to estimate or try to determine whether advertising has a significant effect on smoking prevalence by comparing advertising expenditures and smoking prevalence over time, correct?

A. Correct.

Q. And a number of them control for certain other variables, correct?

A. Correct, yes.

Q. And would you agree that if you didn't control for any other variables, such a comparison would be suspect?

A. Very suspect especially price and trend.

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Those are two, and lagged advertising as you have talked about.

Q. Now, some of the studies also control to some degree for the disclosure of information about the adverse health consequences of smoking?

A. Correct, some of them -- well, there's a whole body of studies some of which are related directly to advertising; some of which peripherally look at advertising because they are interested in other aspects like the fairness doctrine or some other -- they end up being advertising studies but that's not their main purpose, but they do fit in advertising. So, yes, they -- anybody looking at the tobacco time series has to recognize the interventions that have occurred '64, '53, possibly the fairness doctrine which received a lot of attention, and some of the add bans and so forth, correct.

Q. Now, let's assume that a model doesn't completely pick up all of this information regarding the adverse effects of smoking.

A. Uh-huh.

Q. In that situation would the following be possible: That, in fact, advertising tends to

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increase smoking, but during the period of increasing advertising, there was also increased knowledge about the harmful effects of smoking. The two of them canceled each other out and -- with the result that smoking prevalence remained constant. With the following result that when the person simply ran his regression analysis, he found that smoking did not increase during the period of increasing advertising and winds up concluding, therefore, that smoking -- that information -- I'm sorry -- that advertising had no positive effect on smoking prevalence whereas, in fact, it was simply canceled out by the negative effect?

MR. WILLIAMS: Hold on. Your question is is it possible that a particular researcher could come to that conclusion? That's your question?

MR. ANGLAND: Yeah.

MR. WILLIAMS: Okay.

BY MR. ANGLAND:

Q. I think I have to go back and start it again now because I have lost track but --

A. I think I understand what you are saying, but let me rephrase it.

Q. Fine.

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THE WITNESS: Should I stop?

MR. WILLIAMS: Well, I mean, the point is is it possible a particular researcher could reach a particular conclusion, anything is possible.

BY THE WITNESS:

A. What you are saying is --

BY MR. ENGLAND:

Q. I asking whether that's a -- whether the situation I just described wherein there was increased advertising expenditures which had -- in fact, had a positive effect on smoking prevalence but there was increased information about the health circum- -- health consequences of smoking which had a negative effect on smoking prevalence. In that situation if one didn't capture the health effects adequately in your -- in one's regression model, you could wind up coming to in that hypothetical the false conclusion that advertising had no positive impact on smoking prevalence; is that right?

A. That's a possible conclusion, but I would think it's unlikely in the case of the tobacco.

Q. And why is that?

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A. The reason is is that we have a period of dramatic decreases in advertising post-71 at a time when health information is increasing, and then a dramatic increase in advertising between '75 and '70. So it's -- so you are getting a case where you cut down the size -- this 30 percent figure I mention in the early '70s.

Starting in the '60s you are getting a trend in the amount of information in the larger society, and so this trend is at work, and meanwhile you vary a lot the advertising effect. So it could be working at subset of the data, but given the big range in the '70s on the data, I am suspicious that would be a factor.

Q. Well, let's talk --

A. It could happen. Logically it could happen.

Q. And let's talk about the '70s.

A. Yes.

Q. You said earlier that the effects of advertising could be lagged, correct?

A. Oh, yes.

Q. You could have different lag periods for different people, correct?

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A. Oh, for different human beings, individuals?

Q. Yes.

A. That's indeed possible.

Q. So the increase or decrease of advertising in year one might have an effect on one person in year three, but that same effect on another person in year five?

A. Correct.

Q. And that would tend to introduce essentially overlapping waves of mathematically -- so that if you picked any given point in time, you say 1979, you would have difficulty, wouldn't you, distinguishing which effect occurred from the change in the early '70s in one direction or the mid-70s from the other direction?

A. Well, yes and no. The reason why I say no is the following: That if I build a model in which there's a distribution of responses across people, then I can get a lag and the aggregate which is where we are talking about a lot of the data that would still display a stable lag aggregate -- aggregate lag that wouldn't necessarily reflect the experience of any one

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person but the average of the people.

So I could still come up with a fairly stable aggregate lag even though at the micro level I am getting very -- a lot of variability, but if the population is roughly the same I have some people -- take your extreme case where some people are going to the respond after one year; some people are going to respond after three. That's the case. I just put a distribution on those two. Then what I am going to do is add up literally the two fractions -- the two pieces of distribution.

So that means I am going to get something like an average of those lags that come out, but I could come up with a lag relationship in the aggregate. It would lead to a more complicated lag relationship probably.

Q. Let's just take an example of a situation. Let's hypothesize that people vary between 2 and 15 years in terms of the point at which information on the subject will have the maximize --

A. Correct, okay.

Q. And let's assume that in year one advertising goes way up and in year three

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advertising goes way down.

A. Uh-huh.

Q. If you move out to say year six --

A. Uh-huh.

Q. -- you will have some people in that period in year six whose smoking or not smoking decision is principally affected by the change in year one --

A. Correct.

Q. -- which I think I said was an increase.

A. Yes.

Q. And you will have other people whose effect -- whose decision is principally affected by the opposite effect in subsequent years?

A. Correct.

MR. WILLIAMS: Joe, I am just going to correct because you continue to assume that advertising does have an effect in all these hypotheticals, right?

MR. ANGLAND: Yes, I -- but just to make it clear, I understand the witness's position that based upon his review of the literature, he believes it has no little or no effect on prevalence, advertising. And what I am doing is

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simply testing to see whether under certain real world -- certain possible situations in the real world --

MR. WILLIAMS: Because they are not real world.

MR. ANGLAND: -- that the parties disagree about whether under those situations, maybe the studies wouldn't be able to accurately determine whether there was an effect. It's not a question. I am just clarifying the record. So no -- I want to make clear to the witness that I am not trying to shove words into your mouth.

MR. WILLIAMS: I understand, but there are a vast array of studies here.

MR. ANGLAND: I understand that.

MR. WILLIAMS: I think it would be better if you could take one and point out --

MR. ANGLAND: No, I am asking about a concept. So I am going to stick with this.

BY THE WITNESS:

A. Should I answer?

BY MR. ANGLAND:

Q. Yes.

A. Well, I mean the answer -- the answer

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to your question is, yes, but you are dealing with a transient. And so if I go to periods where I have like a steady state growth, as I did in the late '80s, for example, look at advertising effects, for earlier periods back in the '50s when Telsur (phonetic) and other people were doing their studies, then I could avoid that problem. But your problem is correct that literally what you are doing is you are adding up two different groups of people. And it's logically possible in the short run that some people would be switching -- responding to the one stimuli and other people would be responding to the other.

As I said earlier, that should show up as a more interesting lag dynamics which people like Schmalansee tried to -- well, Schmalansee is '72. So he is kind of relating before this period, but the post-Schmalansee literature which is big in the wake of Schmalansee tried and they would have picked up like higher order lags. You would have gotten a response where you would have not just one lag but two. So it looks like the effect is going to be -- you know, and there would be a lot of instability in that lag over time. And then you

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would be able to identify the true structure from the periods of more steady growth.

But you are certainly true that if you focused your data only on the period of the transient and you fit a lag like a simple one period lag model, then you could lead to exactly the kind of story you are saying. I am just thinking that in the sample data, that's not the case because of the fact that we have episodes of steady growth and these transient episodes and then we look and see how well -- how stable the model is in fitting that.

Q. One of the issues addressed in this literature I think to some degree is -- or one of the potential problems, let me phrase it that way. You can tell me if you don't think it's a problem -- is whether advertising -- the level of advertising is a cause or a result --

A. Correct.

Q. -- of smoking prevalence?

A. Correct.

Q. Okay. And let me just see if I can characterize the issue properly so to make sure we are talking about the same thing.

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One at least theoretical possibility is that the response of cigarette companies or any other companies when they see declines or when they project declines in the sales of their product would be to try to counter those declines by increasing advertising?

A. That's true, especially for particular brands.

Q. Okay. And it is at least theoretically possible that even if smoking -- I am sorry -- even if advertising did tend to increase smoking prevalence, holding everything else constant, that if the times of increased advertising were in times where it looked like for other reasons smoking was going to go down, that you wouldn't be able to find in the data when you did your regression the impact of advertising on increasing smoking?

A. Correct.

Q. Okay.

A. But let me add that there are methods around for people to try to avoid that problem which just leads to this whole question of simultaneous equations methods, two-stage lead squares and the like where people are worried

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exactly about that spurious causation. So they try to find instruments -- so-called instruments that predict advertising that aren't directly determinant to the outcome.

Q. Okay. In the literature what would you point to as sort of the gold standard in terms of dealing with that problem?

A. Gold standard?

Q. Well, I will phrase it more generally because maybe there's nothing that achieves the gold standard. Which studies deal with that problem best in your view?

A. I would to take a close look at all the studies to make a rank of the very best, but there are studies that worry about it in different ways. For example, well, peripheral but nonetheless -- peripheral to the point of the study, but nonetheless addressed is the paper by Porter that's in the FTC report edited by Eppilito. He is concerned about the endogineity of price and quantity in the market, and he also worries about advertising as being endogenous.

So there are instrumental variable effects. I mentioned Porter only because he is a

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highly respected figure in the industrial organization literature as we speak today and he was when he wrote the paper. He was emerging.

There are a bunch of studies, and I am just trying to rank the one that is best, but as I recall, now I am stretching. I have to look at the paper. Do you have the like Baltagi and the Levin study? They look at advertising among other things. But there are different ways to deal with it. One is by, you know, so-called fixed effect methods, others instrumental variables.

Gold standard, I don't know, but there are attempts to do deal with it. So that's my understanding of the literature. I would have to check that though. I have not -- if you wanted me to, I could take a look at the studies.

Q. That's okay. Thank you.

A. Yeah.

Q. Completely unrelated question.

A. Yes.

Q. Are you familiar with a paper by Dr. Wecker regarding dealing with missing variables in regression analysis and he used -- he used lead-based paints as his -- as his data set?

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A. This is like an '80s paper or something?

Q. I think early '90s.

A. I think I have seen it. I never studied it closely. I am aware of it. I think he gave it at Chicago actually not when was on the faculty, but I don't know it very well.

Q. I am sure Murray is trying to figure out why I am asking the question, and it's simply I literally found it in my Heckman folder, and I think I just misfiled it and it was intended for my Wecker folder, but if it was in there, I thought there was some chance that maybe you had used it for some purpose in this case?

A. No, no, no.

Q. I came across it last night. I couldn't think of what to ask so.

A. No, I have no --

MR. WILLIAMS: Well, that was a great question.

BY THE WITNESS:

A. I have no -- I have no -- no -- not detailed knowledge of it. I am dimly aware of the paper though.

MR. WILLIAMS: You don't know why it's in his

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file though?

THE WITNESS: No, I do not know why it's --

MR. WILLIAMS: Can we move on?

THE WITNESS: That's sort of what I was asking.

MR. GARNICK: I know but I am not going to tell.

BY MR. ANGLAND:

Q. All right. In a couple of different areas here, for example, the area of advertising, you talked about -- and in the area of intervention studies, you talked about results testing for certain things and not being statistically significant?

A. Yes.

Q. Okay. Is it ever reasonable in your opinion to base -- let me back up.

Let's assume that you are trying to determine whether something has a causal effect.

A. Yes.

Q. And you wind up getting a positive coefficient in the sense that a positive coefficient points toward there being causal effect?

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A. Possible.

Q. By hypothesis here.

A. Right, yeah.

Q. But let's assume that the coefficient is non-statistically significant at the 95 percent level.

A. Yes.

Q. Is it ever reasonable to base decision on that information in the sense of saying, well, it's positive, and so I am going to make the judgment that even though it's not statistically significant, I will conclude that the effect exists? Is that ever reasonable?

MR. GARNICK: Objection to form.

THE WITNESS: Should I answer?

BY MR. ANGLAND:

Q. Yes.

MR. WILLIAMS: If you can.

THE WITNESS: What's that?

MR. WILLIAMS: Is it ever reasonable.

BY THE WITNESS:

A. No, no. I understand the question.

The answer is, yes, under certain decision models it's possible to say that's the

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right decision rule, and the reason being that if I want to make the best prediction, then I might want to say in the sense of -- there's a whole literature on this subject starting with this guy Vapnik, it's a book on statistical learning, Vapnik. He gave some lecture here in Chicago in December. The idea is that if I want to pick a rule in the best way to predict something in the future, my purpose is just predicting, that I might want to throw in a variable that wouldn't be quote statistically significant in the standard way but nonetheless might be a quote good predictor in the sense of surviving.

What that's saying really is the following, that there are many different uses of a statistical model, and if my purpose is prediction, I may reach the construction of the model on a different basis than on the basis of saying is there a causal effect. So I would separate prediction from causal analysis. And so in that sense, Vapnik's work is on prediction purely, and the causal analyst is more like is there an effect. So if it's not statistically significant, most people would say it's not an effect, but that

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wouldn't prevent them from saying, well, if I want to do the best predictor and I use it within sample criterion, I could use that effect so.

THE VIDEOGRAPHER: Counsel, I need to switch tapes.

MR. ANGLAND: Okay.

THE VIDEOGRAPHER: Going off the video record at 1:52 p.m. at the end of tape 2.

(WHEREUPON, a recess was had.)

THE VIDEOGRAPHER: Going back on the video record at 1:57 p.m. at the beginning of tape number 3.

BY MR. ANGLAND:

Q. Let me just follow up for a moment on statistical significance. Let's assume we did the following experiment: I had two boxes here, and you know that in one box there are two white balls and in the other box there is one white and one black ball.

A. Uh-huh.

Q. And you get to point to a given box, and I will draw a ball at random and tell you its color.

A. Uh-huh.

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Q. You point to a given box. I draw a ball at random, and it turns out to be white.

A. Uh-huh.

Q. I put it back in the box, close the box. At that point, can you at the 95 percent level of confidence -- do you have 95 percent -- can you predict which box has the two whites with 95 percent statistical significance?

A. 95 percent. Well, I can compute a probability, a posterior probability on the -- but see the trouble is the 95 percent confidence interval is like answering a different experiment.

Q. Well, I'm trying to ask -- I was going to ask two questions. The probability that the box that the ball was picked from --

A. Yeah.

Q. -- is the one with the two whites would be 66 and two-thirds percent, right?

A. Uh-huh.

Q. And if we view that as an experiment to test whether, in fact --

A. Right.

Q. -- to test the hypothesis --

A. That is this the white --

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Q. -- that this is the white ball, that, in fact, would only be significant at the 50 percent --

A. Yeah, a very, very low level of significance, that's correct.

Q. So --

A. Or a high level of significance I should but meaning a bad -- a bad inference would be made on that because it's only one draw from this study.

Q. And obviously in your ideal world you would like to do more draws and make a more -- assuming that you got some prize for picking the right ball?

A. Absolutely, and as soon as I got my first black ball, I would be in business 100 percent.

Q. Absolutely. But this is a tough house and you are only allowed one look at the box.

A. Correct.

Q. And in that situation, do you have a view regarding whether it is more likely than not that the two -- two white balls are in the box from which the one white ball was chosen?

MR. GARNICK: Objection to form, sorry.

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BY THE WITNESS:

A. Well, I mean that's a very -- that's kind of a classical problem in statistical inference that you are just stating, and the question is if I set it up in like an Bayesian decision problem, then I could answer it in certain ways, right. If I set it up as a classical economist or a classical statistician, I would answer it in somewhat different ways.

Partly to answer the question, I guess I would want to know what rule I was operating in unless you want my private way to operate. I would probably put a prior -- a 50/50 and then say, well I updated my prior towards this particular box by drawing the white ball because I think it's more likely.

BY MR. ANGLAND:

Q. I tried to posit it so that there was --

A. A prior.

Q. -- a 50/50 prior.

A. Okay. Fine. So there's a prior, and I believe the prior and there's no problem with that. Then in that particular decision, I mean, are you asking my -- you are asking what exactly from me I

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mean so I --

Q. I am asking if you would have a view regarding whether it's more likely than not; that is, more than 50 percent likely that the box containing the two white balls is the one from which the white ball was chosen?

MR. GARNICK: Objection to form.

BY THE WITNESS:

A. If I really started off with a probability -- see, the problem with this is that really if you think of the information that's there, if I just look at the so-called sample likelihood of this and I draw from which of these two populations, then it's a 50 percent chance in one case of getting a white ball. In the other case, it's like 100 percent chance of getting the white ball.

So if I were being classical about this and saying I am going to sort of think of, well, what's my -- what's my maximum likelihood estimator, again I would just have to write down what the decision cost is of making the judgment because it's not a set answer to that question, I think. At least my understanding of the decision

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theory. I could come up with different answers depending on what I am willing to bring in and what the costs are of the mistake, right.

BY MR. ANGLAND:

Q. Let's assume that there is no cost of a mistake, but you get some reward if you identify the box --

A. Yeah, correct.

Q. -- that contains the two white balls. Which box would you pick?

MR. GARNICK: Objection to form.

BY MR. ANGLAND:

Q. Or would you pick a box?

A. Well, let's assume I am not -- I will break ties. See, the problem with this is that in each case, the balls are equally -- the boxes are equally -- each -- drawing a white ball in one case is like a certain event. In the other case it's 50/50, but it could happen in both states. So if I am going to sort of do a betting odds, I guess I would if I am a Bayesian decision maker under this case, I would probably slip towards the statement that, well, I got -- I mean, there really is a sense that I would want to be almost

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ambiguous on this because of the 50/50 and the other ball and the 50/50 in the draw, but I would probably favor the white balls.

Q. And if -- I am sorry.

A. Go ahead.

Q. And, in fact, assuming I am -- which isn't far off -- uneducated in statistics but just have the sense that -- I come to the conclusion for one way or another that the box with the two white -- that the white ball is chosen from in my view is more likely to be the one that has the two balls, and we do that experiment about 1,000 times and there's money bet on it each time. I am going to come out way ahead, right? So there is a real world answer.

A. Should come out, yes, yeah, as with betting odds. If I were to do this as a repeated trial, that's where I was saying that would be the classical world. Now I can be in a situation repeated trials. I draw this, and I say on average what's the expected gain. That's different from the Bayesian one off thing where I literally just have this one decision, and I get the reward or not get the reward. So I can't really use a law of

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large numbers.

Q. But even when you do it one time, you have a two-thirds probability --

A. Yeah.

Q. -- of the correct answer and getting the reward if you choose the box from which the white ball was drawn --

A. Right.

Q. -- and a one-third probability if you draw it from the other box?

A. If I took the structure such that I had a black ball, then it's trivial.

Q. Right.

A. In which case then I have a lot of information, but yes.

MR. ANGLAND: Okay. I have nothing further.

MR. GARNICK: Dr. Heckman, --

THE VIDEOGRAPHER: Counsel.

EXAMINATION

BY MR. GARNICK:

Q. For the record, my name is Murray Garnick. I represent Philip Morris. I have a couple questions for you as well.

Mr. Angland asked you a series of

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questions about Dr. Viscusi. Do you remember that?

A. Yes, I do.

Q. And you relied upon some of the work by Dr. Viscusi in your expert report, correct?

A. Yes, I did. Yes, I did.

Q. Now, Dr. Viscusi looked at what people believe is the probability of contracting lung cancer from smoking, correct?

A. Correct, what expectations were -- beliefs were of people, yes.

Q. And I believe you mentioned that one may also want to look at what people believe is the probability or beliefs of contracting lung cancer among nonsmokers?

A. Yes.

Q. And how would you compare those two numbers?

A. Based on?

Q. Well, you said that you would want to look at what people believed is the risk of contracting lung cancer from smokers?

A. Yes.

Q. And you might also want to look at --

A. For smokers you mean?

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Q. For smokers. And you might also want to look at the perception of contracting lung -- people's perception of contracting lung cancer among nonsmokers, right?

A. Yes.

Q. And what would -- how would you go about comparing those two numbers?

A. Well, if I found that one set of numbers was bigger than the other in the population -- I am not sure what you are saying. I mean, I would probably find that people who are nonsmokers -- people would expect that the number would be lower, the probability of contracting lung cancer, but you are not asking me that. You are saying to compare this with something.

Q. Let me give you an example.

A. Yeah.

Q. Let's say that you ask people about their expectation from contracting -- of contracting lung cancer for smokers and they -- strike that.

Let's say that you ask people what they believe is the probability that nonsmokers will contract lung cancer.

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A. Yes.

Q. And let's say that they say that the number there is 30 percent?

A. Yes.

Q. Okay. And that would be an overestimation, correct?

A. Oh, I would think so for lung cancer alone, for sure.

Q. Now, if you asked the same people what they believe is the probability of contracting lung cancer among smokers --

A. Yes.

Q. -- and they said 90 percent?

A. That would be an extreme overestimate, yes.

Q. Now, in a sense the relative risk of exposed over nonexposed would be three, correct?

A. Yeah, so, yeah.

Q. Okay. Would it be appropriate on that basis -- I believe you estimated the relative risk of contracting lung cancer to be around 10 percent?

A. Yeah, the odds ratio.

Q. Around ten, right?

MR. ANGLAND: Object to the form.

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BY THE WITNESS:

A. P over one minus P but yes, yeah.

BY MR. GARNICK:

Q. Would you -- would you consider it appropriate to consider that ten that you estimated and compare it to the three that we just talked about?

A. I could compare the two, yeah.

Q. Would that be an appropriate comparison in your view?

A. Appropriate for what purpose?

Q. To determine if people overestimate the risks or underestimate the risks of smoking?

A. Well, it wouldn't be appropriate relative to the decision theory that we were talking about earlier in the deposition about making choices. I mean, what's relevant is do people have a higher probability of doing one thing versus another, and so knowing that in one case it was three to one or in one case it was ten to one, the judgment would still be made with those probabilities that people would still switch away from smoking.

So in that sense the numbers wouldn't

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matter all that much so long as they were ordered in a way that it was three to one or ten to one or a hundred to one. I mean, they would both point you in the same direction of people switching against it, but I am not sure I am getting the drift of your question.

Q. In that hypothetical that I gave you where people would estimate that the risk of contracting lung cancer among nonsmokers is 30 percent and the risk of contracting lung cancer among smokers is 90 percent, --

A. Yes.

Q. -- would you regard that the people overestimate or underestimate the risks of smoking -- the risk of contracting lung cancer from smoking?

A. Well, there is a case --

MR. ANGLAND: Object to the form. You may answer.

BY THE WITNESS:

A. Okay. But there is a case where what you would have would be a systematic overstatement of the probability of a particular disease to renormalize everything and for the decision problem

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of whether or not people smoke, we don't know; but it seems likely it would be sort of overestimating still their decision -- their response. I don't think I am answering your question though. I somehow seem to be evading it.

BY MR. GARNICK:

Q. I think you have answered it.

A. Okay.

Q. Thank you.

MR. WILLIAMS: Nothing further.

THE WITNESS: That's it?

THE VIDEOGRAPHER: Going off the video record at 2:09 p.m. at the end of tape number 3.

FURTHER DEPONENT SAITH NOT.

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UNITED STATES DISTRICT COURT

EASTERN DISTRICT OF NEW YORK

BLUE CROSS AND BLUE SHIELD OF NEW JERSEY, INC., et al.,

Plaintiffs,

vs.

PHILIP MORRIS, INCORPORATED, et al.,

Defendants.

) C.A. No. 98 Civ. 3287 (JBW)

I hereby certify that I have read the foregoing transcript of my deposition given at the time and place aforesaid, consisting of Pages 1 to 243, inclusive, and I do again subscribe and make oath that the same is a true, correct and complete transcript of my deposition so given as aforesaid, and includes changes, if any, so made by me.

JAMES HECKMAN, Ph.D.

SUBSCRIBED AND SWORN TO before me this day of , A.D. 200 .

Notary Public

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I, ANDREA L. CARTER, a Notary Public within and for the County of Cook State of Illinois, and a Certified Shorthand Reporter of said state, do hereby certify:

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That the foregoing deposition transcript was reported stenographically by me, was thereafter reduced to typewriting under my personal direction and constitutes a true record of the testimony given and the proceedings had;

That the said deposition was taken before me at the time and place specified;

That I am not a relative or employee or attorney or counsel, nor a relative or employee of such attorney or counsel for any of the parties hereto, nor interested directly or indirectly in the outcome of this action.

IN WITNESS WHEREOF, I do hereunto set my

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hand and affix my seal of office at Chicago, Illinois, this 22nd day of February, 2001.

Andrea L. Cate

Notary Public, Cook County, Illinois.

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